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CULTURE VALUE OF MATHEMATICS AND ASTRONOMY II.

WM. W. PAYNE.

In the first part of this article, found on page 585 of this publication, we called attention to the way mathematics has been thought of to some extent, when its branches are being pursued in the schools, and, sometimes, when estimates of its culture value are under consideration.

We emphasized the study of mathematics as a means of rigorous mental discipline, because such study furnishes a field for systematic training with steps of logical sequence that are satisfactorily exact and uniquely strong. We also ventured to suggest that the study of mathematical truth for its own sake, instead of seeking its knowledge for business advantage in practical life, would furnish a motive to enlist mental energy that would be peculiarly effective. As illustrations of this fact, a few of the early scholars in mathematics were named, the methods of their work briefly outlined, and the success that crowned their efforts summarized, using Newton as the great example of scholarly possibility from a profound and almost life-long study of mathematics. In this connection we did not say the half that might be told of this great man, bearing on the means and the methods of his intellectual training, for the simple reason that some of our readers might think that the sources of Newton's greatness had not been rightly estimated. Newton is sometimes spoken of as a genius in mathematics, meaning that he had, by nature, intuitive insight and a spontaneous grasp in mathematical thinking far beyond the average range of the scholarship of his times, and that a genius can not be compared with a student who must work long and hard for every thing he gets in his plodding mental growth. This position is partly wrong and partly right. There are those in every line of scholarship that specialize exhaustively, and that, as a consequence stand in the front rank

of discovery in the realm of new thought, or, those who have a power, almost at will, to do wondrously and almost intuitively the large things that occupy their minds. In this mental condition, however explained, the mathematician is no exception. As an illustration of it, we think of the most extraordinary and inconsistent life of the celebrated Cardan. History says, he was "a gambler, if not a murderer; he was also an ardent student of science, solving problems which had long baffled all investigation; at one time in his life he was devoted to intrigues which were a scandal even in the sixteenth century, at another he did nothing but rave on astrology, and yet at another time he declared that philosophy was the only subject worthy of man's attention. His was a genius closely allied to madness."

We are not considering this kind of genius, but that other which, with good natural gifts, makes its way through mountains of difficulty and opposition by the dint of hard work. There are plenty of such in every age. History is replete with most entertaining and stirring accounts of the deeds and lives of those who have had a genius of the better and the higher kind. This phase has been sufficiently indicated in what has already been said in the introductory part of this article.

In this connection it seems best to notice what has been recently said in a carefully prepared paper,* by a prominent educator belonging to one of the eastern universities. An extract from this paper is as follows.

"Pure mathematics is concerned with the investigation of the logical consequences of certain exactly statable postulates or hypotheses—such, for instance, as the postulates upon which arithmetic and analysis are founded, or such as the postulates that lie at the basis of any type of geometry. For the pure mathematician, the truth of these hypotheses or postulates depends, not upon the fact that physical nature contains phenomena answering to the postulates, but solely upon the fact that the mathematician is able, with rational consistency, to state these assumed first principles, and to develop their consequences. Dedekind, in his famous essay, 'Was Sind und Was Sollen die Zahlen,' called the whole numbers 'freie Schöpfungen des Menschlichen Geistes'; and, in fact, we need not enter into any discussion of the psychology of our number concept in order to be able to assert that, however we men first came by our con-

*The Sciences of the Ideal," an address delivered before the St. Louis Congress of Arts and Science by Professor Joseph Royce of Harvard University. *Science* Oct. 7,p. 449.

ception of the whole numbers, for the mathematician the theory of numerical truth must appear simply as the logical development of the consequences of a few fundamental first principles, such as those which Dedekind himself, or Peano, or other recent writers upon this topic, have, in various forms, stated. A similar formal freedom marks the development of any other theory in the realm of pure mathematics. Pure geometry, from the modern point of view, is neither a doctrine forced upon the human mind by the constitution of any primal form of intuition, nor yet a branch of physical science, limited to describing the spatial arrangement of phenomena in the external world. Pure geometry is the theory of the consequences of certain postulates which the geometer is at liberty consistently to make; so that there are as many types of geometry as there are consistent systems of postulates of that generic type of which the geometer takes account. As is also now well known, it has long been impossible to define pure mathematics as the science of quantity, or to limit the range of the exactly statable hypotheses or postulates with which the mathematician deals to the world of those objects which, ideally speaking, can be viewed as measurable. For the ideally defined measurable objects are by no means the only ones whose properties can be stated in the form of exact postulates or hypotheses; and the possible range of pure mathematics, if taken in the abstract, and viewed apart from any question as to the value of given lines of research, appears to be identical with the whole realm of the consequences of exactly statable ideal hypotheses of every type.

One limitation must, however, be mentioned, to which the assertion just made is, in practise, obviously subject. And this is, indeed, a momentous limitation. The exactly stated ideal hypotheses whose consequences the mathematician develops must possess, as is sometimes said, sufficient intrinsic importance to be worthy of scientific treatment. They must not be trivial hypotheses. The mathematician is not, like the solver of chess problems, merely displaying his skill in dealing with the arbitrary fictions of an ideal game. His truth is, indeed, ideal; his world is, indeed, treated by his science as if this world were the creation of his postulates a 'freie Schöpfung.' But he does not thus create for mere sport. On the contrary, he reports a significant order of truth. As a fact, the ideal systems of the pure mathematician are customarily defined with an obvious, even though often highly abstract and remote, relation to the structure of our ordinary, empirical world. Thus the various algebras which

have been actually developed have, in the main, definite relations to the structure of the space world of our physical experience. The different systems of ideal geometry, even in all their ideality, still cluster, so to speak, about the suggestions which our daily experience of space and of matter give us. Yet I suppose that no mathematician would be disposed, at the present time, to accept any brief definition of the degree of closeness or remoteness of relation to ordinary experience which shall serve to distinguish a trivial from a genuinely significant branch of mathematical theory. In general a mathematician who is devoted to the theory of functions, or to group theory, appears to spend little time in attempting to show why the development of the consequences of his postulates is a significant enterprise. The concrete mathematical interest of his inquiry sustains him in his labors, and wins for him the sympathy of his fellows. To the question, 'Why consider the ideal structure of just this system of object at all?' 'Why study various sorts of numbers, or the properties of functions, or of groups, or the system of points in projective geometry?'—the pure mathematician in general, cares to reply only, that the topic of his special investigation appears to him to possess sufficient mathematical interest. The freedom of his science thus justifies his enterprise. Yet, as I just pointed out, this freedom is never mere caprice. This ideal interest is not without a general relation to the concerns even of common sense. In brief, as it seems at once fair to say, the pure mathematician is working under the influence of more or less clearly conscious philosophical motives. He does not usually attempt to define what distinguishes a significant from a trivial system of postulates, or what constitutes a problem worth attacking from the point of view of pure mathematics. But he practically recognizes such a distinction between the trivial and the significant regions of the world of ideal truth, and since philosophy is concerned with the significance of ideas, this recognition brings the mathematician near in spirit to the philosopher.

Such, then, is the position of the pure mathematician. What, by way of contrast, is that of the philosopher? We may reply that to state the formal consequences of exact assumptions is one thing; to reflect upon the mutual relations, and the whole significance of such assumptions, does indeed involve other interests; and these other interests are the ones which directly carry us over to the realm of philosophy. If the theory of numbers belongs to pure mathematics, the study of the place of the number concept in the system of human ideas belongs to philos-

ophy. Like the mathematician, the philosopher deals directly with a realm of ideal truth. But to unify our knowledge, to comprehend its sources, its meaning, and its relations to the whole of human life, these aims constitute the proper goal of the philosopher. In order, however, to accomplish his aims, the philosopher must, indeed, take account of the results of the special physical science; but he must also turn from the world of outer phenomena to an ideal world. For the unity of things is never, for us mortals, anything that we find given in our experience. You can not see the unity of knowledge; you can not describe it as a phenomenon. It is for us now, an ideal. And precisely so, the meaning of things, the relation of knowledge to life, the significance of our ideals, their bearing upon one another —these are never, for us men, phenomenally present data. Hence the philosopher, however much he ought, as indeed he ought, to take account of phenomena, and of the results of the special physical sciences, is quite as deeply interested in his own way, as the mathematician is interested in his way, in the consideration of an ideal realm. Only, unlike the mathematician, the philosopher does not first abstract from the empirical suggestions upon which his exact ideas are actually based, and then content himself merely with developing the logical consequences of these ideas. On the contrary, his main interest is not in any idea or fact in so far as it is viewed by itself, but rather in the inter-relations, in the common significance, in the unity, of all fundamental ideas, and in their relations both to the phenomenal facts and to life! On the whole, he, therefore, neither consents, like the student of a special science of experience, to seek his freedom solely through conformity to the phenomena which are to be described; nor is he content, like the pure mathematician, to win his truth solely through the exact definition of the formal consequences of his freely defined hypotheses. He is making an effort to discover the sense and the unity of the business of his own life.

It is no part of my purpose to attempt to show here how this general philosophical interest differentiates into the various interests of metaphysics, of the philosophy of religions, of ethics, of esthetics, of logic. Enough—I have tried to illustrate how, while both the philosopher and the mathematician have an interest in the meaning of ideas rather than in the description of external facts, still there is a contrast which does, indeed, keep their work in large measure asunder, viz., the contrast due to the fact that the mathematician is directly concerned with develop-

ing the consequences of certain freely assumed systems of postulates or hypotheses; while the philosopher is interested in the significance, in the unity and in the relation to life, of all the fundamental ideals and postulates of the human mind.

Yet not even thus do we sufficiently state how closely related the two tasks are. For this very contrast, as we have also suggested, is, even within its own limits, no final or perfectly sharp contrast. There is a deep analogy between the two tasks. For the mathematician, as we have just seen, is not evenly interested in developing the consequences of any and every system of freely assumed postulates. He is no mere solver of arbitrary ideal puzzles in general. His systems of postulates are so chosen as to be not trivial, but significant. They are, therefore, in fact, but abstractly defined aspects of the very system of eternal truth whose expression is the universe. In this sense the mathematician is as genuinely interested as is the philosopher in the significant use of his scientific freedom. On the other hand, the philosopher, in reflecting upon the significance and the unity of fundamental ideas, can only do so with success in case he makes due inquiry into the logical consequences of given ideas. And this he can accomplish only if upon occasion, he employs the exact methods of the mathematician, and develops his systems of ideal truth with the precision of which only mathematical research is capable. As a fact, then, the mathematician and the philosopher deal with ideal truth in ways which are not only contrasted, but profoundly interconnected. The mathematician, in so far as he consciously distinguishes significant from trivial problems, and ideal systems, is a philosopher. The philosopher, in so far as he seeks exactness of logical method, in his reflection, must meanwhile aim to be, within his own limits, a mathematician. He indeed, will not in future, like Spinoza, seek to reduce philosophy to the mere development, in mathematical form, of the consequences of certain arbitrary hypotheses. He will distinguish between a reflection upon the unity of the system of truth and an abstract development of this or that selected aspect of the system. But he will see more and more that, in so far as he undertakes to be exact, he must aim to become, in his own way, and with due regard to his own purposes, mathematical; and thus the union of mathematical and philosophical inquiries, in the future, will tend to become closer and closer.

So far, then, I have dwelt upon extremely general considerations relating to the unity and the contrast of mathematical and philosophical inquiries. I can well conceive, however, that the

individual worker in any one of the numerous branches of investigation which are represented by the body of students whom I am privileged to address, may at this point mentally interpose the objection that all these considerations are, indeed, far too general to be of practical interest to any of us. Of course, all we who study these so-called normative sciences are, indeed, interested in ideas, for their own sakes—in ideas so distinct from, although of course also somehow related to, phenomena. Of course some of us are rather devoted to the development of the consequences of exactly stated ideal hypotheses, and others to reflecting as we can upon what certain ideas and ideals are good for, and upon what the unity is of all ideas and ideals. Of course if we are wise enough to do so, we have much to learn from one another. But, you will say, the assertion of all these things is a commonplace. The expression of the desire for further mutual co-operation is a pious wish. You will insist upon asking further: "Is there just now any concrete instance in a modern type of research which furnishes results such as are of interest to all of us? Are we actually doing any productive work in common? Are the philosophers contributing anything to human knowledge which has a genuine bearing upon the interests of mathematical science? Are the mathematicians contributing anything to philosophy?"

These questions are perfectly fair. Moreover, as it happens, they can be distinctly answered in the affirmative. The present age is one of a rapid advance in the actual unification of the fields of investigation which are included within the scope of this present division. What little time remains to me must be devoted to indicating, as well as I can, in what sense this is true. I shall have still to deal in very broad generalities. I shall try to make these generalities definite enough to be not wholly unfruitful.

We have already emphasized one question which may be said to interest, in a very direct way, both the mathematician and the philosopher. The ideal postulates, whose consequences mathematical science undertakes to develop, must be, we have said, significant postulates, involving ideas whose exact definition and exposition repay the labor of scientific scrutiny. Number, space, continuity, functional correspondence or dependence, group-structure—these are examples of such significant ideas; the postulates or ideal assumptions upon which the theory of such ideas depends are significant postulates, and are not the mere conventions of an arbitrary game. But now what con-

stitutes the significance of an idea, or of an abstract mathematical theory? What gives an idea a worthy place in the whole scheme of human ideas? Is it the possibility of finding a physical application for a mathematical theory which for us decides what is the value of the theory? No, the theory of functions, the theory of numbers, group theory, have a significance which no mathematician would consent to measure in terms of the present applicability or non-applicability of these theories in physical science. In vain, then, does one attempt to use the test of applied mathematics as the main criticism of the value of a theory of pure mathematics. The value of an idea, for the sciences which constitute our division, is dependent upon the place which this idea occupies in the whole organized scheme or system of human ideas. The idea of number, for instance, familiar as its applications are, does not derive its main value from the fact that eggs and dollars and star-clusters can be counted, but rather from the fact that the idea of numbers has those relations to other fundamental ideas which recent logical theory has made prominent—relations, for instance, to the concept of order, to the theory of classes or collections of objects viewed in general, and to the metaphysical concept of the self. Relations of this sort, which the discussions of the number concept by Dedekind, Cantor, Peano and Russell have recently been brought to light—such relations, I say constitute what truly justified Gauss in calling the theory of numbers a 'divine science.' As against such deeper relations, the countless applications of the number concept in ordinary life, and in science, are, from the truly philosophical point of view, of comparatively small moment. What we want, in the work of our division of the sciences, is to bring to light the unity of truth, either, as in mathematics, by developing systems of truth which are significant by virtue of their actual relations to this unity, or, as in philosophy, by explicitly seeking the central idea about which all the many ideas cluster.

Now, an ancient and fundamental problem for the philosophers is that which has been called the problem of the categories. This problem of the categories is simply the more formal aspect of the whole philosophical problem just defined. The philosopher aims to comprehend the unity of the system of human ideas and ideals. Well, then, what are the primal ideas? Upon what group of concepts do the other concepts of human science logically depend? About what central interests is the system of human ideals clustered? In ancient thought Aristotle already approached this problem in one way. Kant, in the eighteenth

century, dealt with it in another. We students of philosophy are accustomed to regret what we call the excessive formalism of Kant, to lament that Kant was so much the slave of his own relatively superficial and accidental table of categories, a d that he made the treatment of every sort of philosophical problem turn upon his own schematism. Yet we can not douht that Kant was right in maintaining that philosophy needs, for the successful development of every one of its departments, a well-devised and substantially complete system of categories. Our objection to Kant's over-confidence in the virtues of his own schematism is due to the fact that we do not now accept his table of categories as an adequate view of the fundamental concepts. The efforts of philosophers since Kant have been repeatedly devoted to the task of replacing his scheme of categories by a more adequate one. I am far from regarding these purely philosophical efforts made since Kant as fruitless, but they have remained, so far, very incomplete, and they have been held back from their due fulness of success by the lack of a sufficiently careful survey and analysis of the processes of thought as these have come to be embodied in the living sciences. Such concepts as number, quantity, space, time, cause, continuity, have been dealt with by the pure philosophers far too summarily and superficially. A more thoroughgoing analysis has been needed.

But now, in comparatively recent times, there has developed a region of inquiry which one may call by the general name of modern logic. To the constitution of this new region of inquiry men have principally contributed who began as mathematicians, but who, in the course of their work, have been led to become more and more philosophers. Of late, however, various philosophers, who were originally in no sense mathematicians, becoming aware of the importance of the new type of research, are in their turn attempting both to assimilate and to supplement the undertakings which were begun from the mathematical side. As a result, the logical problem, of the categories has today become almost equally a problem for the logicians of mathematics and for those students of philosophy who take any serious interest in exactness of method in their own branch of work. The result of this actual co-operation of men from both sides is that, as I think, we are today, for the first time, in sight of what is still, as I freely admit, a somewhat distant goal, viz., the relatively complete rational analysis and tabulation of the fundamental categories of human thought. That the student of ethics is as

much interested in such an investigation as is the metaphysician, that the philosopher of religion needs a well-completed table of categories quite as much as does the pure logician, every competent student of such topics ought to admit. And that the enterprise in question keenly interests the mathematicians is shown by the prominent part which some of them have taken in the researches in question. Here, then, is the type of recent scientific work whose results most obviously bear upon the tasks of all of us alike.

A catalogue of the names of the workers in this wide field of modern logic would be out of place here. Yet one must, indeed, indicate what lines of research are especially in question. From the purely mathematical side, the investigations of the type to which I now refer may be viewed (somewhat arbitrarily) as beginning with that famous examination into one of the postulates of Euclid's geometry which gave rise to the so-called non-Euclidean geometry. The question here originally at issue was one of a comparatively limited scope, viz., the question whether Euclid's parallel-line postulate was a logical consequence of the other geometrical principles. But the investigation rapidly develops into a general study of the foundations of geometry—a study to which contributions are still almost constantly appearing. Somewhat independently of this line of inquiry there grew up, during the latter half of the nineteenth century, that reexamination of the bases of arithmetic and analysis which is associated with the names of Dedekind, Weierstrass and George Cantor. At the present time, the labors of a number of other inquirers (among whom we may mention the school of Peano and Pieri in Italy, and men such as Poincaré and Couturat in France, Hilbert in Germany, Bertram Russell and Whitehead in England and an energetic group of our American mathematicians —men such as Professor Moore, Professor Halsted, Dr. Huntington, Dr. Veblen and a considerable number of others) have been added to the earlier researches. The result is that we have recently come for the first time to be able to see, with some completeness, what the assumed first principles of pure mathematics actually are. As was to be expected, these principles are capable of more than one formulation, according as they are approached from one side or from another. As was also to be expected, the entire edifice of pure mathematics, so far as it has yet been erected, actually rests upon a very few fundamental concepts and postulates, however you may formulate them. What was not observed, however, by the earlier, and especially by the philosoph-

ical, students of the categories, is the form which these postulates tend to assume when they are rigidly analyzed.

This form depends upon the precise definition and classification of certain types of relations. The whole of geometry, for instance, including metrical geometry, can be developed from a set of postulates which demand the existence of points that stand in certain ordinal relationships. The ordinal relationships can be reduced, according as the series of points considered is open or closed, either to the well-known relationship in which three points stand when one is between the other two upon a right line, or else to the ordinal relationship in which four points stand when they are separated by pairs; and these two ordinal relationships, by means of various logical devices, can be regarded as variations of a single fundamental form. Cayley and Klein founded the logical theory of geometry here in question. Russell, and in another way Dr. Veblen, have given it its most recent expressions. In the same way, the theory of whole numbers can be reduced to sets of principles which demand the existence of certain ideal objects in certain simple ordinal relations. Dedekind and Peano have worked out such ordinal theories of the number concept. In another development of the theory of the cardinal whole numbers, which Russell and Whitehead have worked out, ordinal concepts are introduced only secondarily, and the theory depends upon the fundamental relation of the equivalence or non-equivalence of collections of objects. But here also a certain simple type of relation determines the definitions and the development of the whole theory.

Two results follow from such a fashion of logically analyzing the first principles of mathematical science. In the first place, as just pointed out, we learn *how few and simple are the conceptions and postulates* upon which the actual edifice of exact science rests. Pure mathematics, we have said, is free to assume what it chooses. Yet the assumptions whose presence as the foundation principles of the actually existent pure mathematics an exhaustive examination thus reveals, show by their fewness that the ideal freedom of the mathematician to assume and to construct what he pleases, is indeed, in practise, a very decidedly limited freedom. The limitation is, as we have already seen, a limitation which has to do with the essential significance of the fundamental concepts in question. And so the result of this analysis of the bases of the actually developed and significant branches of mathematics, constitutes a sort of empirical revelation of what categories the exact sciences have practically found

to be of such significance as to be worthy of exhaustive treatment. Thus the instinctive sense for significant truth which has all along been guiding the development of mathematics, comes at least to a clear and philosophical consciousness. And meanwhile the essential categories of thought are seen in a new light.

The second result still more directly concerns a philosophical logic. It is this: Since the few types of relations which this sort of analysis reveals as the fundamental ones in exact science are of such importance, the logic of the present day is especially required to face the questions: *What is the nature of our concepts of relations?* What are the various possible types of relations? Upon what does the variety of these types depend? What unity lies beneath the variety?

As a fact, logic, in its modern forms, viz., first that symbolic logic which Boole first formulated, which Mr. Charles S. Peirce and his pupils have in this country already so highly developed, and which Schroeder in Germany, Peano's school in Italy and a number of recent English writers, have so effectively furthered —and secondly, the logic of scientific method, which is now so actively pursued, in France, in Germany and in the English-speaking countries—this whole movement in modern logic, as I hold, is rapidly approaching *new solutions of the problem of the fundamental nature and the logic of relations.* The problem is one in which we are all equally interested. To De Morgan in England, in an earlier generation, and, in our time, to Charles Peirce in this country, very important stages in the growth of these problems are due. Russell, in his work on the 'Principles of Mathematics,' has very lately undertaken to sum up the results of the logic of relations, as thus far developed, and to add his own interpretations. Yet I think that Russell has failed to get as near to the foundations of the theory of relations as the present state of the discussion permits. For Russell has failed to take account of what I hold to be the most fundamentally important generalization yet reached in the general theory of relations. This is the generalization set forth as early as 1890, by Mr. A. B. Kempe, of London, in a pair of wonderful, but too much neglected, papers, entitled, respectively, 'The Theory of Mathematical Form,' and 'The Analogy between the Logical Theory of Classes and the Geometrical Theory of Points.' A mere hint first as to the more precise formulation of the problem at issue, and then later as to Kempe's special contribution to that problem, may be in order here, despite the impossibility of any adequate statement."

If we had the space for it, the whole address of which the above is an extract, would have been very profitable reading for any one interested in the culture value of the study of pure mathematics, to see just what ground its several branches cover, their relations to one another, and especially the relation of pure mathematics to the whole realm of philosophy including the field of modern logic.

This thought of the advantage of mental culture to be found in the realm of the applied mathematics is still more important and more complex than that of pure mathematics, if one but realizes the endless field of application that is now open in every direction. This phase of our subject which includes astronomy will be considered at another time.

THE WATSON ASTEROIDS.

BURT L. NEWKIRK *

James C. Watson was born in the province of Ontario, Canada, January 28, 1838. He graduated at the University of Michigan in 1857, and became Professor of Astronomy and Director of the Observatory of that institution six years later. He was one of our most prominent astronomers, having written a book on theoretical astronomy which is still very widely used in the United States. He died in 1880, at the age of forty-two years. During the course of his scientific career he discovered twenty-two asteroids, and at his death left a sum of money as an endowment fund, the income from which should be used to pay for certain investigations and computations which it is necessary to make in order that these asteroids may not be lost to the scientific world.

Beside the well-known greater planets the Sun's system contains a host of very minute planets called asteroids. They revolve about the Sun in the space between the orbits of *Mars* and *Jupiter*. The first one of these to be discovered was found upon the first night of the nineteenth century, the night of January 1, 1801. This is the asteroid *Ceres*, and it is the brightest, and presumably the largest, of the group, having a diameter of 600 miles. Since the introduction of photographic methods in the search for these bodies, their discovery has been made comparatively easy, and something over five hundred of them have

* Read at the meeting of the Society held in Berkeley January 30, 1904.

already been found, every year adding twenty or thirty to the list. At the time of Watson's death, in 1880, less than two hundred had been found. The smallest of these bodies are probably nothing more than great rocks, ten or fifteen miles in diameter. Smaller asteroids than these undoubtedly exist, but are so exceedingly faint as to elude discovery.

Of all the planets of the Sun's system these asteroids offer the greatest difficulties in the matter of the investigation of their orbits. We say, roughly speaking, that the planets move about the Sun in elliptical orbits; but, more accurately speaking, none of the bodies composing the Sun's system move in ellipses. According to Newton's law of gravitation, every particle of matter attracts every other particle. If the Sun were attended by one planet only, this planet would move in an ellipse, but since each planet is attracted not only by the Sun, but also by all other planets, its path is a curve of corresponding complexity. Since the Sun's attraction is generally by far the most powerful of all the forces acting upon any one planet, it is convenient to think of the actual orbit described by a planet as a "disturbed ellipse," as we express it. We picture to ourselves an ellipse which nearly coincides with the actual orbit, and in which the planet would move if the attractive forces of the other planets should at some particular instant cease to operate. The departure of the true orbit from this assumed ellipse, due to the disturbing action of the other planets is called the perturbation of the planet under consideration. The only reason for taking an ellipse as a starting-point in the discussion is one of convenience. A circle, which is a simpler curve from a mathematical point of view, might answer the purpose in some cases better than an ellipse, and for other purposes it is advantageous to take as a starting-point a curve of greater complexity. I refer to the "periplegmatic" curves used by Gyldén, which represent the path of the planet throughout a long period of years much better than any ellipse could, but seem to possess no advantages in tracing the planet's motion for a short period of time. It is a comparatively simple matter to compute an elliptic orbit, but to investigate the deviations from this orbit—i. e. the perturbations of a planet—is a task requiring in some cases a tremendous amount of labor and study.

The mathematical difficulties of the problem are such that no general method can be employed for computing the orbits of all the planets. Each planet must be treated with reference to the special difficulties which it presents, and this necessitates an in-

telligent and discriminating study of the various methods used in investigations of this nature, their advantages and their limitations.

The method employed in any particular case must not only be mathematically correct, but it must also be capable of yielding the desired results with the required degree of accuracy and with a minimum of numerical computation. The method of most general application is one developed by Hansen, and modified by Hill, Newcomb, and others. Newer methods which possess special advantages in certain cases have been developed by Gyldén, Bohlin, and Brendel. These latter methods are, however, comparatively untried, and it has been found necessary here in the asteroid work at Berkeley to revise Bohlin's method to some extent, before employing the formulæ given. Upon opening correspondence with Bohlin, whose method has been used on some asteroids here, Professor Leuschner found that Bohlin himself had arrived at the same conclusion and was at work upon a revision of his theory.

In the case of the asteroids the problem presents special difficulties because of the proximity of *Jupiter*, which is the largest planet, and exerts a very powerful disturbing force. In most cases, in fact, unless a high degree of accuracy is required, the effect of all the other planets combined is a negligible quantity as compared with the perturbations produced by *Jupiter*. Difficult as the problem is, it is however, absolutely necessary to compute the perturbations if we would keep the asteroids from retiring again into the oblivion from which their discoverers drew them. It is not possible to predict the motion of an asteroid ten or fifteen years in advance with sufficient accuracy to permit of its being found again at the end of that time without serious difficulty unless this work is done.

In the light of these remarks, Professor Watson's object in endowing the twenty-two asteroids discovered by himself will be clear. If their perturbations are not derived, his asteroids will be lost to the world in a few years, but, thanks to the fund he has bequeathed for this purpose, it will be possible to predict their motion for fifty or seventy-five years in advance with sufficient accuracy to enable astronomers of the future to find them again when they are wanted without serious difficulty. One of the twenty-two must be excepted from this statement. The asteroid *Aethra*, whose original path passed close to that of *Mars*, has suffered such violent perturbation that the form of its orbit has been greatly changed, and it has not been identified

since, in spite of the diligent search which has been made for it. The tracing of the motion of this planet by means of a special mathematical discussion, which will be an exceedingly difficult matter, is to be undertaken by Miss Hobe, who is now engaged with me upon the perturbations of the other asteroids, under the direction of Professor Leuschner.

Since Professor Watson's death, in 1880, the trustees have been trying to carry out the desire expressed in his will, by preparing tables by means of which the motion of these asteroids can be predicted for, say, fifty years in advance, with sufficient accuracy to permit of their being readily found. Up to two years and a half ago, when the work was undertaken by this department, considerable has been done, but little was ready for publication. Since that time the perturbations of ten planets have been computed here in Berkeley, and those of two more are nearing completion. Five others are to be treated by Bohlin's method, the work on these being already under way: the four remaining asteroids have been made the subject of investigation by other astronomers in Europe and America.

Perhaps the most important result of the investigations of the orbits of the Watson asteroids is the light thrown upon the whole subject of asteroid orbits and the methods best adapted to the various cases that arise. The treatment of twenty-two asteroids yields data which will be very valuable in the solution of one of the great problems which now confronts mathematical astronomy,—namely, that of providing tables by means of which the position of any one of the known asteroids may be found without excessive labor.

Before our tables for finding the planets in future years can be finished it will be necessary to compare the results of our investigations with observations. It is possible by this means greatly to improve the results of the numerical work. For this purpose the photographic telescope and Repsold measuring apparatus will be available. With the help of these instruments we shall be able to observe the positions of the asteroids, and a comparison of observations with theory will lead to a final improvement of the tables before publishing them.

The Watson trustees have, as may be imagined from the long time that has elapsed since Professor Watson's death without the completion of the task, had great difficulty in getting the work done satisfactorily. They have, however, been very well pleased with the progress made here by Drs. Crawford and Ross, under the supervision of Professor Leuschner, and have now

turned the whole work over to the latter to be completed and prepared for publication. The work is being carried on under the auspices of the National Academy of Sciences, and the present Watson trustees are: Professor Simon Newcomb (chairman), Professor Lewis Boss, and Professor W. L. Elkin. It is their intention to have all the results published in due time. It has, however, seemed fitting upon this occasion to offer to those interested in the Berkeley Astronomical Department this brief statement of our connection with the undertaking.

SOLAR AND SIDEREAL TIME.

ARTHUR K. BARTLETT.

It is a fact not generally known that, owing to the difference between solar and sidereal time, the Earth rotates upon its axis once more often than there are days in the year. The Earth performs one complete rotation upon its axis in 23 hours, 56 minutes, and 4.09 seconds of solar time. This is called a "sidereal day," because in that time the stars appear to complete one revolution around the Earth. But, as the Earth advances almost a degree eastward in its orbit, in the time that it turns eastward around its axis, it is easy to understand that just one rotation never brings the same meridian around from the Sun to the Sun again; so that the Earth requires as much more than one complete rotation upon its axis to complete a "solar day" as it has gone forward in that time. It will be obvious therefore, that in every natural or solar day, the Earth performs one complete rotation upon its axis, and the 365th part of another rotation. Consequently, in 365 days, the Earth turns 366 times upon its axis; and as every rotation of the Earth upon its axis completes a "sidereal day," there must be 366 sidereal days in a year.

The same considerations will apply to our neighbor worlds of the solar system, and since the rotation of any planet upon its axis is the length of a sidereal day at that planet, the number of sidereal days will always exceed the number of solar days by one, let that number be what it may, one rotation being always lost in the course of an annual revolution. This difference between the solar and sidereal days may be illustrated by referring to a watch or clock. When both hands set out together, at twelve o'clock for instance, the minute hand must travel more

than a whole circle before it will overtake the hour hand, that is, before they will again come into conjunction.

As the star-sphere turns a little more than once around in twenty-four hours, if we observe the heavens, night after night at the same time, we shall notice precisely the same kind of change as when we look at the heavens hour after hour on the same night. If we look at the stars at ten o'clock on any night and note their position, and we again note the position of the stars at eleven o'clock on the same night; then, if afterwards we examine the stars night after night at ten o'clock, we shall find that at the end of about fifteen days they have at this hour the same position that they had on the first night at eleven o'clock —that is, they have advanced by just one hour's motion. In a month or thereabouts, they will be found to have advanced by two hours' motion. In a year they advance by twenty-four hours' motion—that is, by one complete rotation—so that they have resumed their original positions, and this fact explains why, in the course of a year, the star-sphere (in reality the Earth) turns around once oftener than there are days in the year, or 366 times. While during a year the Sun rises 365 times, a star rises 366 times. The latter will therefore during the year have risen at every hour of the day and night.

The late Professor Proctor, in a very instructive article on "How to Learn the Stars," says: "The amount by which the stars have advanced each night on the position they held at the same hour on the preceding night is by no means so small as is, perhaps, commonly imagined. This may be easily tested. Let there be an upright of any sort a few yards to the north of the observer's station, and let him notice the exact hour when a star (at a fair height above the horizon) appears from behind the edge of this upright. At this hour on the next night he will find that, as seen from the same station, the star is about two moon's breadths past the upright's edge. The observer should look through a fixed tube placed in the same position on each night."

The rotation of the Earth determines the length of the day, and may be regarded as one of the most important elements in astronomical science. It serves as a universal measure of time, and forms the standard of comparison for the revolutions of the celestial bodies, for all ages, past and to come. As a recent writer has well remarked, "Theory and observation concur in proving, that among the innumerable vicissitudes that prevail throughout creation, the period of the Earth's diurnal rotation

is immutable." This statement is not strictly correct and needs to be qualified somewhat in the light of some recent discoveries regarding tidal action and its effects in retarding the Earth's rotation, but except from this cause, which is very slow and hardly noticeable in its operation, the length of the sidereal day has not varied as much as the one-hundredth of a second during the last two thousand years. At present we can only say that the change, if any change has occurred since astronomy became an exact science, has been too small to be detected, and as Professor Young remarks, "Probably it has not changed by the one-thousandth part of a second, though of that we can hardly be sure."

Time may be defined as a definite, measured portion of eternal duration, and for many purposes, astronomers find it much more convenient to reckon by the stars than by the Sun. Sidereal time, of course, would not answer for the ordinary affairs of life, since its noon, when the Vernal Equinox is on the meridian, comes at all hours of the night and day at different seasons of the year. We may define this kind of time, at any moment, as the hour-angle of the Vernal Equinox at that moment, and nearly every almanac and text-book on astronomy contains data from which sidereal time and mean solar time may be easily converted into each other.

BATTLE CREEK, Mich.

GREENWICH ASTROGRAPHIC WORK.

WM. W. PAYNE.

Since we have known that the Royal Observatory at Greenwich was to engage with other prominent observatories in the photographic survey of the heavens, we have expected large and helpful results to be the outcome from this source particularly.

It will be remembered that in 1887, was held in Paris, an international congress on Astronomical Photography, at which fifty-six representative astronomers from all parts of the world were present. This first meeting convened by invitation of the French Academy of Sciences, in the month of April of that year, was for the purpose of considering the question of making a photographic map of the heavens by the united effort of many observatories situated in different parts of the world so that all parts of the sky might be photographed under circumstances as favorable as possible.

At that meeting a scheme was approved for this general photographic map that should consist of two sets of photographs each covering two degrees by two degrees on a scale of one millimeter to one minute of arc. One of these photographs was to have an exposure of forty minutes, which was to be used in making the map of the whole sky, commonly called the Astrographic Chart, and the other photograph was to have short exposures of six minutes, three minutes and a supplementary one of twenty seconds, from which a catalogue of reference stars could be made. Each set is taken in duplicate, the centers of one series being at the corners of the other series. A réseau of cross lines five millimeters apart is photographed on each plate to aid in determining the position of stars.

The Astronomer Royal of England was a delegate to the Paris Congress, and at its close he made a report to the Board of Visitors, urging that the Royal Observatory at Greenwich take its share in this scheme which would, in a few years, greatly extend the knowledge of the places of the fixed stars. This important recommendation was supported by such men as Warren de la Rue and Sir William Huggins, and it was soon decided that such coöperative work should be undertaken at Greenwich, and that a suitable photographic telescope for this kind of work should be procured.

Instructions were immediately given to Sir Howard Grubb, as to the details of the new instrument, with directions to proceed at once with its construction.

The new telescope was received in May, 1890, and it was erected in an 18-foot dome which had been before completed for it. During the year following the instrument was brought into working order, but it was kept in the experimental stage until December 1891. While in this condition, among other things, photographs were taken to test the distortion of the object-glass, photometric experiments with various exposures were made with wire and perforated metal screens in front of the object-glass, different photographic plates were tried and the various adjustments of the instrument were perfected.

In December 1891 the regular series of photographs for the Greenwich Zones; extending from 64° north to the Pole were begun, on the plan of photographic exposures already described above.

Volume I. of this Astrographic Catalogue containing a full account of the work done by the Greenwich section of this photographic survey of the sky has just been received. The photo-

graphs taken in the Greenwich Zones have all been made and reduced under the direction of W. H. H. Christie, the Astronomer Royal, and the record so published makes a heavy quarto volume of 738 pages.

In the detail of this work the first thing noticed is the distribution of the photographs for the Astrographic Chart and Catalogue in the part of the sky assigned to Greenwich. Beginning with 65° and 66° in north declination the number of plates used for each was 80, the space covered by each plate being eighteen minutes in right ascension, from center to center. From 67° to 71°, the interval was twenty minutes, and the number of photographs seventy-two. From 71° to 74°, the interval was twenty-four minutes, and the number of photographs were sixty for each of the degrees included. It will readily be seen that when the 90° is reached only one photograph is needed, and by this plan the entire Greenwich Zone would be uniformily covered with equal care.

The amount of work required to secure all these plates in so careful a way that they may be accurately measured later no one can realize who has not had a hand in that kind of astronomical study, long enough to be acquainted with many of the varying and attendant circumstances which the practical astronomer has to face in prosecuting his work. The following table will give some idea of it, in a general way, if we think of it as the limiting numbers of the photographs taken each year in doing the work on the Greenwich Zone:

Year.	Nos.	Year.	Nos.
1890	1— 25	1895	2423—2969
1891	26— 230	1896	2970—3334
1892	231— 717	1897	3335—3808
1893	718—1728	1898	3809—4219
1894	1729—2422	1899	4220—4758

The next important step was the measures of the photographs. After some experimental work, and some discussion of methods and results, systematic work on the measurement of the plates began in October 1894.

What is said here in a few words means greatly more than the ordinary reader will be likely to suppose, because the whole field of this kind of work is very new, and not very easy in kind. The experimental measures of some of the photographs began in 1893, the first being made on photographs of the Pleiades, which showed that the optical distortion was negligible to a distance of sixty minutes from the center of the plate, and very small at the distance of eighty minutes from the center. An account of

this experimental work has already been published in the *Monthly Notices* of the Royal Astronomical Society of England for December 1894.

To hasten the measurement of the plates and to bring together measures of the same star on two overlapping plates a duplex micrometer was arranged by which two such plates could be measured simultaneously. The work began in this way in February 1895, and all measures from this time forward were done in this way.

At the June meeting of the Astrographic Committee at Paris, it was decided to duplicate the measurement of the plates of the Catalogue, the plates being reversed in the micrometer for the second series of measures. The zones that had been measured before this date were 65°, 66° and 67° and parts of 64° and 68°; after this care was taken that the same measurer should do the work in both direct and reversed positions on plates of six-minute and three-minute exposures until June 1896, after which time different persons were employed.

As our readers already know, this work well illustrates a typical method in a new line, it seems helpful to notice the instrument used although it is not a new one. It is, however, one constructed for this particular kind of work, and a general description of it with a frontispiece illustration are therefore given.

The instrument was constructed on the lines laid down by the International Astrographic Congress in 1887, and its general form is shown in the cut just referred to. It has a thirteen-inch photographic telescope and a parallel ten-inch visual guiding telescope in steel tubes firmly connected, mounted equatorially in the German form. The focal lengths of both telescopes are the same, 135·1 inches, so that the scale of a plate placed in the focal plane is one millimeter to one minute of arc, approximately. The photographic telescope is corrected for spherical and chromatic aberration for rays near the Fraunhofer's line G. It is arranged to carry a plate·16 centimeters square, with special provision for each focusing and orientation.

The eye-piece of the ten-inch visual telescope is mounted in cross slides that permits of the observation of a guiding star at a distance of forty-five minutes of arc from the center of the field.

This mounting will allow a motion of the telescope for one and one half hours each side of the meridian without reversing the telescope. Looking at the cut of the telescope one sees that heavy counterpoise is necessary on account of the distance of the two telescopes from the polar axis. Anti-friction rollers and

heavy counterpoises make the movement of the telescopes easy in right ascension. Similar provisions care for the thrust of the polar axis. The driving clock is placed inside the stand and controlled electrically by a seconds' pendulum. The detector of the control is similar in principle to that used in Sir David Gill's form; and the system of connection by differential wheels was devised by Sir Howard Grubb.

The photographs for the Astrographic Chart and Catalogue were all taken with the telescope west of the pier, and in most cases, within one hour of the meridian, the guiding telescope being below the photographic telescope when the instrument is pointing north. The photographs whose measures are given in this volume were all taken between April 2, 1892 and May 28, 1900. The focal adjustment placed the center of the plate somewhat within the focus which was about forty minutes of arc from the center, in order to equalize the definition as far as practicable over the field.

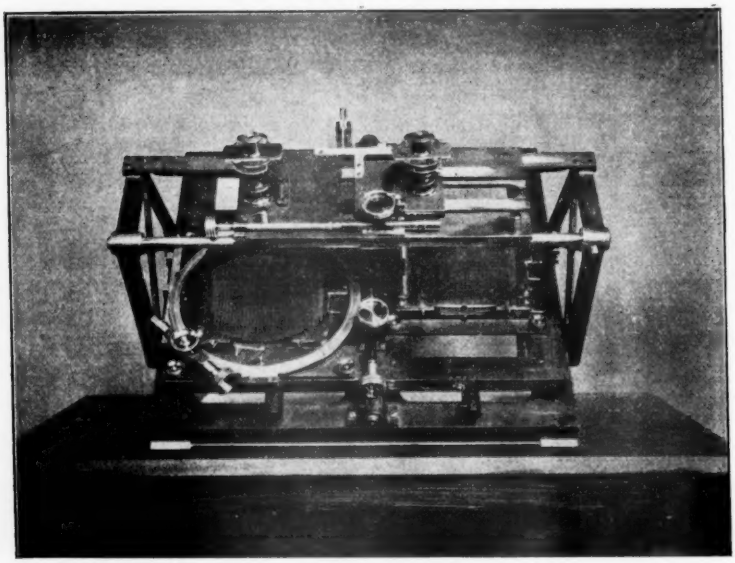
In July 1897 the object-glass was removed and cleaned and replaced without altering the adjustments. During the whole period no alteration was made in the focal adjustment, or, in the tilt of the object-glass, though the latter was verified from time to time. A list of these verifications is given, and a test of the uniformity of the field at different distances from the center is also shown in a satisfactory way.

The method of taking the photographs, the measurement of the photographs and the measurement of diameter and determination of photographic magnitude are other important features of this Catalogue.

The detailed work of choosing the proper kinds of photographic plates, the determination of the réseau as to breadth of lines, size of square, and method of impression on the film are all instructive reading for those who are interested in the practical details of this work.

As we come to the measurement of the photograph the duplex micrometer with the glass diaphragm claims interested attention. A cut of this is herewith presented. This micrometer was brought into use in January 1895, and with it nearly all the measures for the Catalogue were made. The essential feature of the instrument is that two different plates on which the same portion of the sky is photographed are viewed and measured at once. "The plates are placed in a frame movable in the direction of the γ co-ordinate, and the carrier of one of the plates can be adjusted so that the images of the same star on the two plates

are very approximately on the same horizontal line. Two microscopes carried on a horizontal slide move perpendicularly to the direction in which the frame moves, and their distance apart can be readily adjusted so that when the image of a star on one plate is at the center of the field of one microscope the image of the same star on the second plate is at, or very near, the center of the field of the second microscope." The various working parts of the instrument will be easily understood.



DUPLEX MICROMETER.

The glass diaphragm with its two scales at right angles are so divided that one interval corresponds to one hundredth of a réseau interval of three seconds of arc. The star's image is placed accurately at the intersection of the two scales, and the position of the réseau lines relative to it is read off on the scales by estimation to one thousandth part of a réseau interval, which means in the sky three-tenths of a second of arc.

From what has already been said, it has been noticed that the arrangement of measures adapted for this work is such that the sky is divided into zones in declination one degree wide, and the measures of the same star on overlapping portions of two plates are given side by side. It is plain that the work of measurement

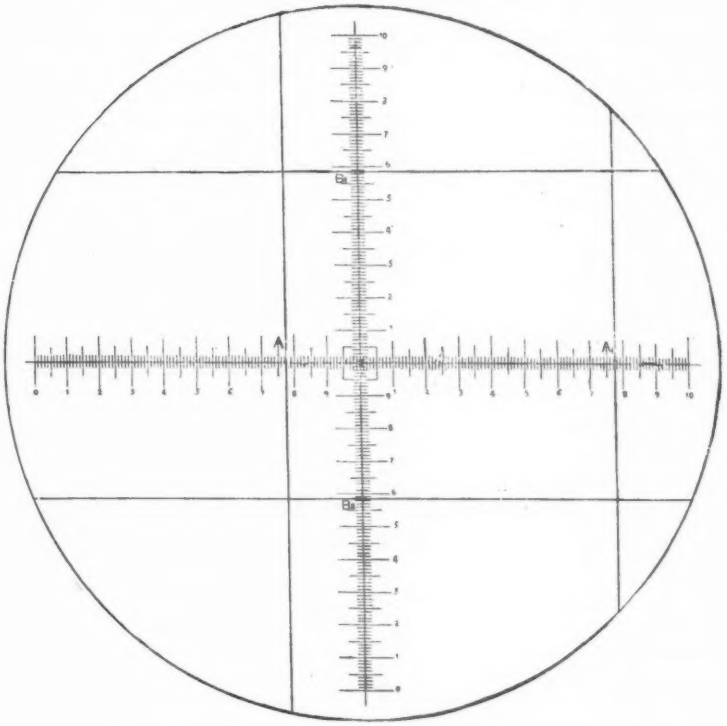


DIAGRAM OF GLASS DIAPHRAGM.

by the aid of the duplex micrometer would be considerably hastened. The following cuts show approximately how the

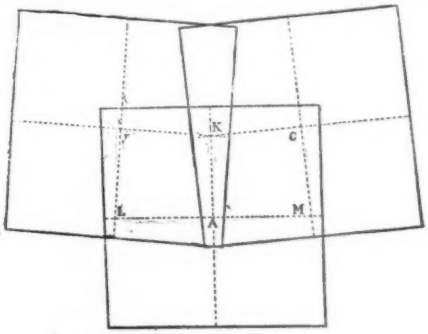


DIAGRAM I.

plates were arranged in this overlapping process. For example a star near the center of A would also be shown on the right hand bottom corner of B, the left hand bottom corner of C, and on the top right and left hand corners respectively of two plates in a lower zone. In Diagram I., A, B, C, are the centers of the three plates A K, L A M, B L, B K, and C M, C K, are the central réseau lines. A K, B L, and C M, are the projections of the meridians.

In Diagram II., the division of the sky is shown. A, B, C, K, L, M, correspond to the same letters in Diagram I. D is the center of the adjacent plates in the same zone as A, E in the center of the third plate in the same zone as C and B.

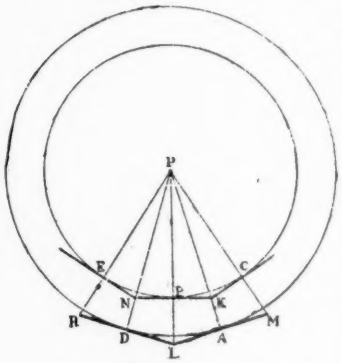


DIAGRAM II.

The plan of measuring the plates is of course a very important feature of the work. Each plate is measured in two positions the second being when the plate is turned through 180° giving a direct and reversed position with careful independent readings in each. "The readings on the glass scale of the two réseau lines between which the stars are contained are given in the same horizontal line, the measures of three-minute images are placed below those of the six-minute images. The mean includes the correction for *runs*, and is formed by applying the proportional part of the difference of the readings of the réseau lines on the two sides of the glass scale." In this way a weighted mean is at once found from the measures, the unit of measure is simple and the diameters of the star images are quickly and reliably found. The specimen page of the measuring book is interesting in these particulars.

The last point that we can notice in the hasty examination of this volume is the determination of the photographic magnitudes of the stars whose images are found on the photographic plates. We have spoken of the way in which the measures of the star-images on the photographic plates were made, indicating that the result in every case was the mean of the two measures at least. The unit employed in the printed results is one two-thousandths of the réseau interval or fifteen hundredths of a second of arc. If this value is unity, then four would be six-

tenths of a second of arc, and ten would be one and one half seconds of arc and so on; the diameters thus being expressed as integers.

No difficulty presents itself in thus measuring fairly bright stars near the center of the plate. At a distance of forty minutes from the center the star images begin to be sensibly elongated in the radial direction, so it was necessary to take the mean of the horizontal and vertical diameters and set down this as an equivalent of a circular diameter. In the case of the faint star-images on the plates, "The actual diameter may be the same as that of a somewhat brighter star, but the image is gray and not black. Allowance is made by the measures for the want of density in the images, and also for the shading off at the edge, and the diameter set down is that of an equivalent black image, *i. e.* one which would contain the same amount of silver deposit. In all measures made since 1897, great care has been taken to secure uniformity in these estimates, but in the measures before this date, *i. e.* in the direct position between Dec. + 64° and + 68°, the diameters of the very faint stars have not been reduced sufficiently to make them strictly comparable with those brighter stars having black images."

When the best measures for the photographic images of the faint stars on the plates have been secured, the next step is to relate these results to the standard photometric measures in units of star magnitude. In this work the photometric scale used is given by Professor Pickering in the Harvard Annals of faint stars near certain variables which lie in the Greenwich zones. Other aids of photometry are also used in making needed comparisons for the sake of converting the photographic measures of star images into stellar magnitudes consistent with the standard scale of photometry. This must be done by finding some empirical law connecting the two. The formula expressing this relation is worked out in an article in the *Monthly Notices* of the Royal Astronomical Society, Vol. 53, pp. 125-146, and is as follows, m equaling magnitude, d, measured diameter, C and n being constants:—

$$m = C - n\sqrt{d}$$

We have been unusually full in regard to some of the details of this interesting volume of modern method, mainly because the methods are new, and we are sure that the amateurs and astronomers on this side of the sea will be as much interested as we can be, to know something of the details of how these things are done by experts elsewhere.

The remainder of the volume is generally in the line of methods that do not need special reference.

American astronomers engaged in any way in astrographic work will have a library in itself if they are fortunate enough to become possessors of the entire catalogue and charts that finally will make up the whole survey of the heavens. We very much hope that Goodsell Observatory will be able to secure the volumes embracing the entire catalogue and the charts that will cover the Greenwich zones if we are unable to possess the parts to be obtained from the sources.

COMMON'S 60-INCH TELESCOPE.

EDWARD C. PICKERING.

An important part of the work of the Harvard College Observatory for the last quarter of a century, and of the writer personally, has been the determination of the light of the stars. About one hundred thousand measures of four thousand stars, including all those visible to the naked eye in Cambridge, and all those of the sixth magnitude and brighter, north of declination $-30°$, were made in 1879 to 1881. The telescope used in this work had an aperture of only two inches. A similar instrument, with an aperture of four inches, has been in use here and in Arequipa, since 1882. More than a million settings, on nearly sixty thousand stars, in all parts of the sky from the North to the South Pole, have been made with it. Numerous standards of the tenth magnitude and brighter are thus provided. In 1899, a telescope of twelve inches aperture was mounted horizontally, and used in a similar manner. Since then, four hundred thousand measures of about eleven thousand stars have been made, thus furnishing standards of the twelfth magnitude and brighter.

As this Observatory has not heretofore owned a very large telescope, we have had to rely on the courtesy of our friends for measures of the fainter stars. By means of an appropriation from the Rumford Committee, and with the coöperation of the Directors of the Yerkes, Lick, McCormick, and Halsted Observatories, several of the largest telescopes in the world have taken part in a determination of standard magnitudes of very faint stars. A request for a telescope of the largest size, to be mounted at Harvard, has not hitherto been made, since the atmospheric conditions in the eastern part of the United States and in Europe

are not so favorable to the best work as in certain selected stations in the tropics. By a modification of the twelve-inch telescope described above, good measures can be made of the stars, even if their images are bad. By reducing the image of the real star, instead of that of the artificial star, more accurate measures may be made, certain sources of constant error eliminated, and since both images are faint when compared, defects in them are rendered imperceptible. In this way, even so large an object as the planet Mars has been satisfactorily compared with an artificial star.

A reflecting telescope of sixty inches aperture was constructed by the late A. A. Common, and for several years has been idle. From its great aperture it should show extremely faint stars, and would be especially adapted to measuring their light. Some years ago, an attempt was made to purchase this telescope, but the means of the Observatory would not then permit. In 1902, the anonymous gift of $20,000 was received, and it has supplied several urgent needs of the Observatory. Representing these facts to Professor Turner of Oxford, during his recent visit to Harvard, he recognized the importance of utilizing so valuable an instrument, and that the nature of the observations and other conditions were favorable to securing valuable results. He therefore wrote to Mr. T. A. Common, with the result that this Observatory has purchased the telescope on such liberal terms that Mr. Common may fairly be regarded as having contributed a large portion of the cost.

Steps are being taken for packing and transferring the instrument at once to Cambridge. It is hoped that in a few weeks the telescope may be received and mounted, and that observations to supply one of the great wants of Astronomy, a measure of the light of the very faint stars, can then begin. The work of many years has supplied this want for the brighter stars, and may now be extended to the faintest objects within the reach of human knowledge.

We have here another illustration of the valuable results which may be obtained in Astronomy, from a combination of favorable circumstances. It is still another result of the utmost importance derived from a gift made without restrictions. It affords an opportunity to make useful the greatest work of one of the most successful of the makers of large telescopes. It secures the liberal aid of his family through the friendly assistance of a brother astronomer. All have been brought together through a form of investigation which appears especially suited

to these particular conditions.

HARVARD COLLEGE OBSERVATORY,
Circular No. 83, August 18, 1904.

VARIATION OF LATITUDE.

WM. W. PAYNE.

An intelligent reader of this journal recently asked if there is anything new in regard to the variation of latitude which might be of service to him in speaking of this modern astronomical discovery in a public address, now in the course of preparation.

In answering this query we have thought that it might interest our readers generally if we gave a very brief review of the steps by which astronomy came into possession of this important discovery, as well as to point out some of the more significant consequences likely to follow in regard to the physics of the Earth as a whole.

Since the latitude of any place on the Earth's surface is the distance of it from the equator either north or south, it is evident that the latitude of all places would be constant, or without change in long periods of time, if the equator itself has a fixed and unvarying position constantly on the surface of the Earth. But this imaginary line which we call the Earth's equator is a line measuring the circumference of the Earth everywhere ninety degrees from two points called the geographic poles of the Earth. These poles of the Earth are the ends of the axis of the daily rotation of the Earth. Now it is evident that if this imaginary line within the Earth, which we call its axis of daily rotation, has uniformly a constant place in the spheroid, then the points on the surface called poles will not change, the equator can not change and the latitude of places on the Earth's surface will not change as long as the first named condition prevails. But if this axis of rotation changes its place within the Earth it is probable that such a change will be made to keep the rotation symmetrical in regard to mass. In view of the great mass of the Earth, its rapid rotation; and, as the mathematician would say, its very large moment of rotational energy, one can easily imagine what a tremendous force would be required to change the plane of rotation, or to make the Earth rotate unsymmetrically. These things direct the physicist's study, at once, to the interior structure of the Earth. They have also given a challenge to the astronomer to tell if he can, whether or not the

Earth is rotating about one fixed axis of symmetry as time goes on indefinitely.

The fact that the poles of the Earth may change their places on the surface of the Earth within very narrow limits has been thought of long ago. Euler about the middle of the eighteenth century, or a little later, investigated what would be the effect of such movements if they should take place. As Turner has pointed out in his "Modern Astronomy," page 199: "He found that a rigid body like the Earth, which was spinning about on an axis once a day with nearly complete steadiness, might have a slight 'wabble' which would mean that the north pole was in motion, but that, if so, the motion would complete a circuit every ten months. There seemed to be no doubt about this result, and astronomers examined their observations of latitude to see whether there was any change of ten months period. *None was found*, although long series of observations were cut up into chapters of ten months and added together to magnify any small disturbance; and after several attempts of this kind, the question was regarded as settled in the negative. The north pole did not move at all. So confident did astronomers become on this point that when Mr. Chandler, who ultimately demonstrated the real facts so clearly, found an apparent movement of the pole by observation with the Almucantar in 1885, he himself thought he must have made some mistake, and did not follow up the matter."

The instrument with which Dr. Chandler made this observation was one of his own planning and radically different from similar ones of its kind. We have repeatedly spoken of the Almucantar in this publication at other times.

Not long after Dr. Chandler's observations were made, Dr. Küstner of Berlin published his own made about the same time which also indicated a motion of the north pole. This fact was a fortunate one just at this time for general attention was called to this singular coincidence, and interest was awakened on account of it. There were two ways of study of this important problem. One was to make a series of observations in future years adapted to the problem in hand and then find the result from them. The other was to make a study of observations already made in the past and determine the motion of the pole in that way. German astronomers chose the former method and Dr. Chandler of America selected the latter, feeling sure that if the supposed motion of the pole was real it ought to be plainly revealed in observations that have been made and published, as well as in those

that should be made in the future.

The observations of the past had been examined for this purpose, but the real question was, had those observations been worked out as thoroughly as as they could and should be. Euler's limit of ten months might not be the whole truth. Chandler said "let us give up this limitation, and see whether or not the observations perhaps indicate some other period. Very soon after work began on this plan Dr. Chandler was able to say with certainty that the period was fourteen months instead of ten.

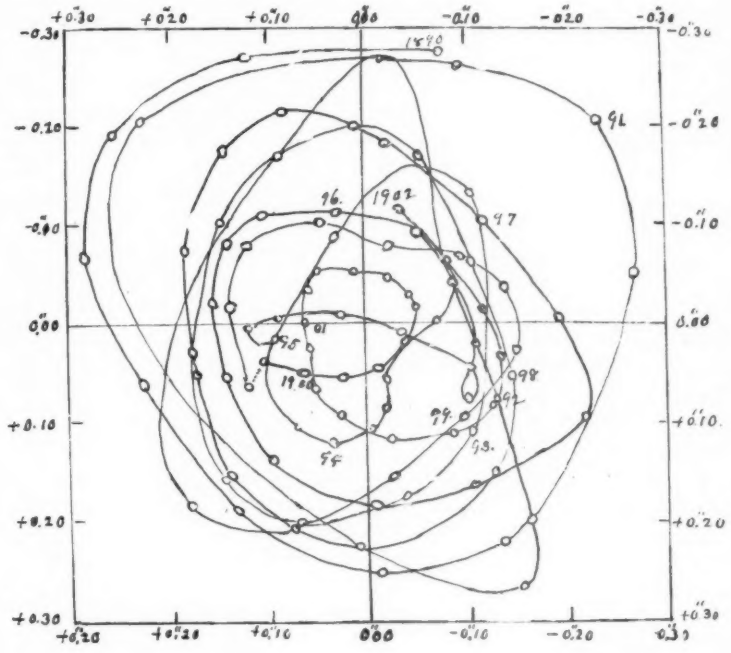
Then followed one of those lively discussions in which one mathematician stands alone with the world of that science against him. The great Euler had said, from a rigid analysis by mathematics, that if such motion of the pole should take place, the period would be ten months. No one could dispute Euler's mathematics. The ablest scholars said so, and told Chandler he must have made some mistake somewhere or somehow in his work. Chandler's reply was heroic and worthy of the metal that great men are made of. He answered in effect that he did not care for Euler's mathematics, the observations plainly showed his result of a fourteen months' period. If Euler said ten months he must be wrong. As Professor Turner of England has tersely said, "It was a dead-lock: Chandler and observation against the whole weight of astronomical opinion and theory." About this juncture of the battle Professor Simon Newcomb of Washington, one of America's brilliant lights in mathematics and astronomy began to look into the problem carefully, and it occurred to him that possibly Euler had made some mistake in assuming, as a condition in the problem that the Earth was a *rigid body*, and possibly, in this direction, lay the discrepancy between theory and observation, Chandler still firmly holding that observation must always decide upon the probability of theory. Not long after this Professor Newcomb and some prominent physicists took the position that the Earth is not a rigid body, and that when allowance is made for the yielding of its stiff mass to stress, ten months' period in its wabble is a minimum period, which may be increased more and more according to the plastic quality of the Earth's mass. Chandler won. His period of fourteen months was the correct one, and his noble discovery brought to science more than he himself anticipated in giving it some data by which to measure the plasticity of the Earth.

The movement of the pole on the surface of the Earth is small,

not greater than about thirty feet from a mean position at any time, so that astronomers do not feel that any serious consequences can come to the common order of things by the change of latitude due to these physical facts.

An illustration of the motion of the north pole for a period from 1890 to 1902 is given herewith, which shows the nature of its periodic motion. Dr. Chandler's view of it is that the movement of the pole is composed of two motions, one *annual* revolution in an ellipse about thirty feet long, but varying in width of position, and the other a revolution in a circle about twenty-six feet in diameter having *a period of about* 428 days which is the fourteen months' period already mentioned.

The accompanying cut is a copy of Professor Th. Albrecht's drawings found in his papers concerning the results of interna-



MOTION OF THE NORTH POLE BETWEEN 1890 AND 1902.

tional latitude work up to the year 1902. The latest of these papers which we have seen was published in 1903 at Berlin, Germany. In this cut it is necessary to combine two drawings

which were easily traced because both were drawn to the same scale.

EXPLOSION OF STARS.*

PROFESSOR A. W. BICKERTON.

Do stars explode? Are the observers of Lick and Yerkes correct when they said that Nova Persei had become a nebula that was expanding at such a rate that no theory of its origin was tenable, but that a star had exploded, been converted into gas, and blown at a velocity of thousands of miles a second to spread itself throughout the entire universe?

Is it conceivable, with the known laws of matter and energy, that a force can be generated great enough to blow a star to pieces? A calculation shows that were the entire star an explosive, it would have to be a score of thousands of times stronger than dynamite. Is there in Nature anything in which such a store of energy exists? This question must undoubtedly be answered in the affirmative, and the source of the energy is the attractive force of gravitation. The force with which the Sun attracts matter, and the enormous distance through which this force extends, gives us an energy so great that, without any original motion, a particle falling from the nearest star upon the Sun would reach it with a velocity of three hundred and ninety miles a second. This velocity would possess an energy hundreds of millions of times greater than that of an express train, and the temperature produced by the stoppage of the motion would excel that of an electric furnace a score of thousands of times.

Hence, in the collision of suns we have an agent that may generate energy sufficient to cause the Sun to explode, but so enormous is the mass of a Sun, that the energy of collision has been shown to be too small to blow the Sun into a nebula; but the probabilities of a direct complete collision between suns is small indeed. Any original motion or any attraction of other bodies acting during their fall towards one another, would tend to make the impact of a tangential character, and it is upon the study of tangential impact that the solution of our problem depends. The velocity with which two suns would sweep past one another would be so great that a slight graze would not stop them. They would fly past one another, scarred by the encounter; but

* *Knowledge and Scientific News*, Nov. 1904.

the portions that lay in one another's path, and that did actually come into collision would be swept from the remainder, would coalesce, and would form a new body in space. The tremendous motion would be converted into heat, and the mass of the new body, if the graze were not deep, might be so small that the explosive pressure produced would blow it into a nebula that would continue to expand with an enormous velocity, and every particle be finally dissipated into free space; in some cases leaving the very universe itself.

It is thus seen that the numbers and distribution of the stars must, on the demonstrated laws of Nature, produce an explosion; and it is highly probable that all the so-called temporary stars that have appeared at intervals in the heavens, usually increasing in brilliancy for some hours, or a day or two, and then gradually disappearing, are caused by partial impacts of stars or, in most cases, of dead suns. For all these bodies have similar spectra crossed with double lines, the one showing recession, and the other approach, indicating the two scarred suns that have struck one another; whilst the brilliant continuous spectrum seen in all new stars, for some time after the outbreak, is due to the mass of flaming gas that must expand at the rate of some millions of miles an hour.

The velocity with which these bodies pass one another would cause the impact to be over in an hour or less; and in this time a body is produced with a higher temperature than that of any ordinary star. This brilliant body would soon expand until the globe of fire would be thousands of times the volume of the Sun.

Hence we need not be surprised that Tycho Brahe's new star grew to be more brilliant than Jupiter, even more brilliant than Venus at quadrature; so intense, in fact, as to be visible at noonday. Nor need we wonder at its disappearance, for the flight of its myriad molecules all travelling from the point where the explosion occurred, would rapidly tend in their radial outrush to become parallel, and the molecules consequently cease to strike one another save at intervals; and as molecules only radiate immediately after encounters, it is obvious that, as these encounters become fewer in number, the luminosity of the mass would lessen and go on lessening until it was absolutely lost to vision.

Herschel has told us that the only possible explanation of the character of the many planetary nebula that he discovered was that they were hollow shells of gas. Every stellar explosion that is produced by a partial impact must result, at one stage of

its history, in a planetary nebula that may be permanent or evanescent according to the attractive power of the new body as compared with its temperature.

Thus evanescent planetary nebulæ would be produced bv slight grazes, whereas a deeper graze might produce a permanent planetary nebula, and still deeper grazes result in a large ratio of the molecules being attracted back, and producing a star in the center of the nebula. Examples of this are comparatively numerous in the celestial vault.

So that our observers were doubtless right in the conclusion they came to that "Nova Persei" was a celestial explosion in which a star had been blown to pieces. And this fragment of the study of impact shows how important an agent impact is in astronomical evolution, for it must be remembered that all kinds of impacts may take place, from a mere graze up to a complete impact. Impacts may take place between dead suns or lucid stars. They may take place between meteoric swarms, or be tween star clusters. The impact of nebulæ may range from a mere graze through deep cuts, up to entire coalescence; and every form of impact save direct center to center must result in rotation, and obviously furnishes an explanation of the spiral character of so many thousands of nebulæ. Again, such vast bodies as the two magellanic clouds may be approaching one another, and after countless ages may impact, and should they strike deep enough into one another, coalescence of a whirling character would result, giving a galaxy of stars ,of a double spiral character, and spreading the poles of the ring with masses of nebulous matter, a configuration that exactly corresponds with the structure of our universe, and hence may we not ask the question, "Is not our visible universe a result of the coalescent impact of two previously existing universes, and if so may not such cosmic systems exist in endless number throughout the infinity of space?"

Such are the lofty conceptions that develop themselves from the study of impact, carried fearlessly to its legitimate conclusions.

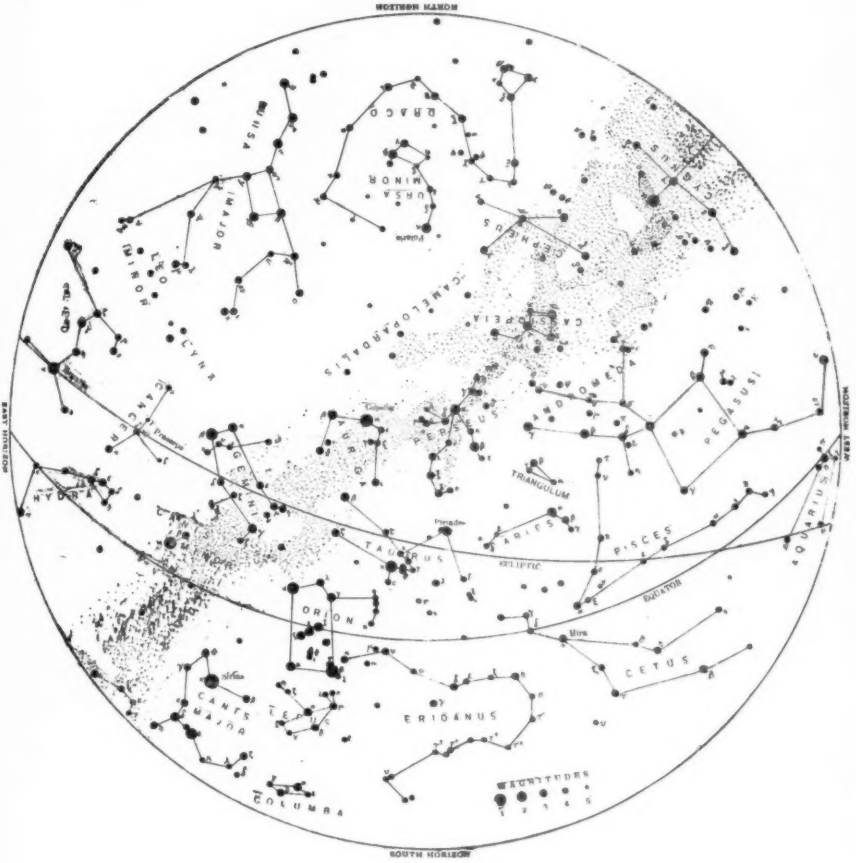
PLANET NOTES FOR JANUARY, 1905.

H. C. WILSON.

Mercury will be morning star during this month, and will be at greatest elongation, west from the Sun 24° 29′ on January 22. The brightness of the planet then will be only 39 as compared with its maximum of 71. On the

morning of January 14 Mercury and Uranus will be in conjunction, Uranus being2° 46′ south of Mercury.

Venus is ascending the slope of the ecliptic and toward the end of the month will be close to the equator and well out from the Sun, so that observing conditions, except for the winter temperature are good. The disk is two-thirds



THE CONSTELLATIONS AT 9 P. M. JANUARY 1, 1905.

lighted and the brilliancy, now about half of that at maximum is rapidly increasing. Venus and the crescent Moon will be in conjunction on the morning of January 9 at 9 o'clock, C. S. T., the Moon passing 2° 12′ to the north of Venus.

Mars will be at quadrature, 90° west from the Sun, January 26. The disk of the planet on Jan. 1 will be between 6″ and 7″ in diameter and will be nine-

tenths illuminated. Its brilliancy then will be 4 as compared with a maximum, for the year 1905, of 37.

Jupiter will be at quadrature, 90° east from the Sun, January 11. No doubt during the past two or three months many of our readers have been watching the changes in the appearance of the belts of this splendid planet as its swift rotation has brought different parts into view night after night. The great red spot of years ago is no longer visible, but the place where it was is marked by the great indentation which it produced in the prominent south temperate belt, showing that there is something of permanence in its character, although its red color has long ago faded out.

Saturn will set too soon after the Sun to be observed during this month.

Uranus is behind the Sun and cannot be observed during this month.

Neptune is just past opposition, and so is in best position for the year. Its position January 1 will be R. A. $6^h\ 28^m\ 53^s$, Decl. + 22° 15′ and February 1, R. A. $6^h\ 25^m\ 25^s$, Decl. + 22° 18′.

PHENOMENA OF JUPITER'S SATELLITES.

1905 [Central Standard Time].

	h	m				h	m		
Jan. 2	8	50	P. M.	III Oc. Dis.	Jan. 14	6	08	P. M.	II Tr. In.
	10	08	"	I Oc. Dis.		8	40	"	II Tr. Eg.
	11	03	"	III Oc. Re.		8	52	"	II Sh. In.
3	7	17	"	I Tr. In.		11	21	"	II Sh. Eg.
	8	37	"	I Sh. In.	16	5	27	"	II Ec. Re.
	9	30	"	I Tr. Eg.	18	8	28	"	I Oc. Dis.
	10	49	"	I Sh. Eg.	19	5	36	"	I Tr. In.
4	4	36	"	I Oc. Dis.		6	57	"	I Sh. In.
	8	07	"	I Ec. Re.		7	50	"	I Tr. Eg.
5	5	18	"	I Sh. Eg.		9	09	"	I Sh. Eg.
	8	23	"	II Oc. Dis.	20	6	28	"	I Ec. Re.
	10	55	"	II Oc. Re.		6	57	"	III Tr. In.
	11	07	"	II Ec. Dis.		9	13	"	III Tr. Eg.
6	4	29	"	III Sh. In.	21	8	47	"	II Tr. In.
	6	19	"	III Sh. Eg.		11	20	"	II Tr. Eg.
7	6	02	"	II Tr. Eg.	23	5	29	"	II Oc. Re.
	6	14	"	II Sh. In.		5	40	"	II Ec. Dis.
	8	43	"	II Sh. Eg.		8	03	"	II Ec. Re.
10	9	11	"	I Tr. In.	26	7	34	"	I Tr. In.
	10	32	"	I Sh. In.		8	53	"	I Sh. In.
	11	34	"	I Tr. Eg.		9	47	"	I Tr. Eg.
11	6	32	"	I Oc. Dis.	27	4	55	"	I Oc. Dis.
	10	03	"	I Ec. Re.		8	24	"	I Ec. Re.
12	5	01	"	I Sh. In.	28	4	16	"	I Tr. Eg.
	6	54	"	I Tr. Eg.		5	34	"	I Sh. Eg.
	7	14	"	I Sh. Eg.	30	5	38	"	II Oc. Dis.
	10	58	"	II Oc. Dis.		8	11	"	II Oc. Re.
13	4	32	"	I Ec. Re.		8	18	"	II Ec. Dis.
	5	06	"	III Tr. Eg.		10	42	"	II Ec. Re.
	8	31	"	III Sh. In.	31	6	43	"	III Ec. Dis.
	10	20	"	III Sh. Eg.		8	14	"	III Ec. Re.

NOTE.—In. denotes ingress; Eg., egress; Dis., disappearance; Re., reappearance; Ec., eclipse; Oc., occultation; Tr., transit of the satellite; Sh., transit of the shadow.

The Moon.

		Phases.	Rises. (Central Standard Time at Northfield. Local Time 13m less.)			Sets.		
1905			h	m		h	m	
Jan.	5	New Moon	7	25	A. M.	5	09	P. M.
	13	First Quarter	11	47	P. M.	1	0	A. M.
	20-21	Full Moon	4	49	"	7	43	"
	27	Last Quarter	12	02	A. M.	11	22	"

Occultations Visible at Washington.

Date. 1905.	Star's Name.	Magnitude.	IMMERSION. Washington M.T.		Angle f'm N pt.	EMERSION. Washington M. T.		Angle f'm N pt.	Duration.	
			h	m	°	h	m	°	h	m
Jan. 2	B.A.C. 5712	6.5	16	20	122	17	15	265	0	55
9	B.A.C. 7919	6.5	8	36	83	9	34	237	0	58
10	Mayer 997	6.4	9	02	59	10	02	260	1	00
14	B.A.C. 764	6.3	6	23	116	7	21	195	0	58
16	D.M. + 14° 657	5.9	4	42	85	5	55	238	1	13
16	γ Tauri	3.9	12	55	152	13	20	197	0	25
18	D.M. + 17° 1182	5.7	14	35	163	15	00	208	0	25
19	D.M. + 17° 1479	6.2	8	00	140	8	46	217	0	46
20	B.A.C. 2649	6.0	6	14	111	7	09	252	0	55
20	5 Cancri	5.9	7	35	119	8	35	247	1	00
22	A Leonis	4.6	11	46	125	13	00	274	1	14
23	*c* Leonis	5.1	9	29	135	10	22	258	0	53
25	*k* Virginis	5.7	13	27	138	14	33	273	1	06
28	B.A.C. 5188	6.5	13	03	117	13	59	278	0	56
29	24 Scorpii	5.0	14	55	190	15	01	201	0	06

COMET AND ASTEROID NOTES.

Ephemeris of Comet Tempel$_2$ (1873 II).

1904		α app.			δ app.			log Δ	Aber.		$1:r^2\Delta^2$
		h	m	s	°	′	″		m	s	
Dec.	1	19	41	27.6	−24	45	36	0.28871	16	9	
	2		45	13.0		42	51	28981		12	
	3		48	58.1		39	44	29092		14	
	4		52	42.9		36	15	29205		17	0.131
	5	19	56	27.4		32	25	29319		19	
	6	20	0	11.5		28	14	29435		22	
	7		3	55.2		23	41	29552		24	
	8		7	38.5		18	48	29671		27	0.126
	9		11	21.3		13	34	29791		30	
	10		15	3.6		8	0	29913		33	
	11		18	45.3		2	5	30036		35	
	12		22	26.4	23	55	51	30161		38	0.122
	13		26	6.9		49	17	30286		41	
	14		29	46 8		42	24	30414		44	
	15		33	26.0		35	12	30542		47	
	16		37	4.4		27	41	30672		50	0.117
	17		40	42.1		19	52	30804		53	
	18		44	19.1		11	44	30936		56	
	19		47	55.2		3	19	31070	16	59	
	20		51	30.5	22	54	37	31206	17	3	0.113
	21		55	5.0		45	37	31342		6	
	22	20	58	38.7		36	21	31480		9	
	23	21	2	11.4	−22	26	49	0.31619	17	12	

EPHEMERIS OF COMET TEMPEL$_2$ (1873 II).—Continued.

1904		α app.			δ app.			log. Δ	Aber.		$1:r^2\Delta^2$
		h	m	s	°	′	″		m	s	
Dec.	24	21	5	43.3	−22	17	0	31760	17	16	0.108
	25		9	14.2	22	6	56	31901		19	
	26		12	44.2	21	56	36	32044		22	
	27		16	13.3	21	46	2	32188		26	
	28		19	41.4	21	35	12	32333		29	0.103
	29		23	8 5	21	24	9	32479		33	
	30		26	34.7	21	12	52	32626		37	
	31		29	59.8	21	1	21	32775		40	
	32	21	33	24.0	−20	49	37	0.32924	17	44	0.098

Ephemeris of Comet *a* 1904 (Brooks)—The following ephemeris is a continuation of that given by A. A. Nijland in *Astr. Nachr.* Nos. 3952 and 3963, and has been computed by H. L. Rice of the U. S. Naval Observatory. The parabolic elements adopted by Nijland are published by him in *A. N.* No. 3952.

The brilliancy of the comet remains practically constant from Oct. 10 to the end of the year, soon after which it will begin to diminish quite rapidly. Observations made with the 26-inch equatorial at the U. S. Naval Observatory, about the middle of October, indicate that good observations of this object might readily be made with the larger instruments during November and December.

EPHEMERIS OF COMET *a* 1904.

G. M. T. 1904		α			δ		log Δ	Br.
		h	m	s	°	′		
Oct.	30.5	12	36	56	+ 43	47.3	0.5999	0.18
Nov.	1.5		37	22	43	58.3	.5986	.18
	3.5		37	46	44	10.2	.5973	.18
	5.5		38	07	44	22.9	.5959	.18
	7.5		38	26	44	36.5	.5945	.18
	9.5		38	42	44	51.0	.5930	.18
	11.5		38	55	45	6.4	.5915	.18
	13.5		39	5	45	22.7	.5899	.18
	15.5		39	11	45	40 0	.5883	.18
	17.5		39	13	45	58.2	.5866	.18
	19.5		39	12	46	17.3	.5049	.18
	21.5		39	7	46	37 2	.5832	.18
	23.5		38	57	46	58.0	.5814	.18
	25.5		38	43	47	19.7	.5796	.18
	27.5		38	23	47	42.3	.5778	.18
	29.5		37	58	48	5.8	.5760	.18
Dec.	1.5		37	27	48	30.2	.5742	.18
	3.5		36	50	48	55.5	.5724	.18
	5.5		36	7	49	21.7	.5705	.18
	7.5		35	17	49	48.7	.5687	.18
	9.5		34	20	50	16.6	.5669	.18
	11.5		33	15	50	45.2	.5651	.18
	13.5		32	1	51	14.5	.5634	.18
	15.5		30	39	51	44.5	.5617	.18
	17.5		29	8	52	15.2	.5301	.18
	19.5		27	27	52	46.5	.5586	.18
	21.5	12	25	37	+ 53	18.3	0.5571	0.18

Comparison with an observation made here Oct. 17 gives as corrections to the ephemeris: $\Delta\alpha = -2^s$, $\Delta\delta = -0'.4$.

Mr. E. I. Yowell, of the Naval Observatory, is making a definitive determina-

tion of the orbit of Comet a 1904 (Brooks), and requests that all yet unpublished observations of it be printed as soon as possible.

The Astronomical Journal.—Supplement to 568.

EPHEMERIS OF ENCKE'S COMET.

(For Berlin Noon continued from page 489.)

1904		α app.			δ app.			log r	log Δ	Aber.	
		h	m	s	°	′				h	m
Oct.	14	0	38	46	+ 28	14.5		0.2248	9.8521	5	55
	15		35	3	28	12.0		2215	8452		50
	16		31	15	28	8.5		2183	8383		44
	17		27	20	28	4.1		2150	8315		39
	18		23	20	27	58.7		2116	8248		33
	19		19	15	27	52.5		2082	8182		28
	20		15	4	27	45.2		2048	8117		23
	21		10	48	27	36.9		2013	8054		19
	22		6	28	27	27.7		1977	7993		14
	23	0	2	3	27	17.3		1941	7932		10
	24	23	57	34	27	5.7		1905	7872		5
	25		53	2	26	53.1		1868	7815	5	2
	26		48	27	26	39.4		1831	7759	4	58
	27		43	49	26	24.4		1793	7706		55
	28		39	8	26	8.2		1754	7654		51
	29		34	25	25	51.1		1714	7604		48
	30		29	41	25	33.1		1675	7555		44
	31		24	55	25	13.7		1635	7507		41
Nov.	1		20	8	24	53.1		1594	7461		38
	2		15	21	24	31.5		1552	7420		35
	3		10	34	24	8.6		1510	7380		33
	4		5	48	23	45.1		1467	7342		30
	5	23	1	3	23	20.6		1424	7305		28
	6	22	56	19	22	55.1		1380	7270		25
	7		51	37	22	28.7		1335	7237		24
	8		46	57	22	1.5		1289	7207		22
	9		42	19	21	33.5		1243	7178		20
	10		37	44	21	5.0		1195	7151		18
	11		33	11	20	35.6		1147	7125		17
	12		28	42	20	5.8		1098	7102		16
	13		24	17	19	35.3		1048	7080		15
	14		19	54	19	4.5		0997	7059		13
	15		15	34	18	33.0		0946	7040		12
	16		11	18	18	1.2		0893	7023		11
	17		7	5	17	29.0		0840	7008		10
	18	22	2	56	16	56.4		0785	6993		10
	19	21	58	49	16	23.7		0730	6978		9
	20		54	47	15	50.9		0673	6965		8
	21		50	47	15	17.8		0615	6953		7
	22		46	49	14	44.3		0555	6942		7
	23		42	54	14	10.6		0495	6931		6
	24		39	4	13	37.1		0433	6921		6
	25		35	17	13	3.6		0371	6912		5
	26		31	31	12	29.7		0307	6904		5
	27		27	47	11	55.5		0242	6896		4
	28		24	3	11	21.3		0175	6888		4
	29		20	21	10	46.9		0106	6880		3
	30		16	40	10	12.3		0.0036	6874		3
Dec.	1		13	0	9	37.5		9.9964	6868		2
	2		9	20	9	2.4		9891	6862		2
	3		5	40	8	27.0		9815	6855		2
	4	21	2	0	7	51.3		9738	6849		2
	5	20	58	21	+ 7	15.4		9.9659	9.6843	4	1

New Asteroids.—The following have been added to the list of new minor planets since our last note:

	Discovered by	at	Local Mean Time.		R. A. h m	Decl. ° ′	Mag.
1904 OY	Götz	Heidelberg	1904 Oct. 3	9 54.8	1 00.4	+18 48	11.7
OZ	Wolf	"	9	12 08.6	2 14.8	+ 6 25	13.2
PA	Götz	"	10	8 09.2	1 01.5	+ 3 58	12.1
PB	"	"	14	12 43 8	2 50.2	+ 5 37	11.0
PC	"	"	14	12 43.8	3 00.8	+ 9 00	12.0
PD	"	"	15	12 43.8	1 26.3	+15 14	12.5
PE	Wolf	"	16	11 30.7	2 54.4	+15 54	12.5
PF	"	"	16	13 39.0	2 32.4	+ 9 19	13.0

VARIABLE STARS.

Approximate Magnitudes of Variable Stars Nov. 10, 1904.

[Communicated by the Director of Harvard College Observatory, Cambridge, Mass.]

Name.	R. A. 1900. h m	Decl. 1900. ° ′	Magn.	Name.	R. A. 1900. h m	Decl. 1900. ° ′	Magn.
T Androm.	0 17.2	+26 26	11.5 *i*	R Camel.	14 25.1	+84 17	12.4*d*
T Cassiop.	0 17.8	+55 14	9	R Bootis	14 32.8	+27 10	8 *i*
R Androm.	0 18.8	+38 1	14	S Librae	15 15.6	−20 2	*s*
S Ceti	0 19.0	− 9 53	11 *d*	S Serpentis	15 17.0	+14 40	12.1*d*
S Cassiop.	1 12.3	+72 5	11.5 *i*	S Coronae	15 17.3	+31 44	13
R Piscium	1 25.5	+ 2 22	10.8*d*	S Urs. Min.	15 33.4	+78 58	7.5*i*
U Persei	1 52.9	+54 20	7.5	R Coronae	15 44.4	+28 28	6
R Arietis	2 10.4	+24 36	9.5 *i*	V "	15 45.9	+39 52	11.5*d*
o Ceti	2 14.3	− 3 26	9.7	R Serpentis	15 46.1	+15 26	9.5*d*
S Persei	2 15.7	+58 8	10 *i*	R Herculis	16 1.7	+18 38	9 *d*
R Ceti	2 20.9	− 0 38	11.2*d*	R Scorpii	16 11.7	−22 42	*s*
U "	2 28.9	−13 35	8.0 *i*	S "	16 11.7	−22 39	*s*
R Trianguli	2 31.0	+33 50	9.5*d*	U Herculis	16 21.4	+19 7	12
R Persei	3 23.7	+35 20	7.5 *i*	W Herculis	16 31.7	+37 32	8.5
R Tauri	4 22.8	+ 9 56	9.5 *i*	R Draconis	16 32.4	+66 58	8.4 *i*
S "	4 23.7	+ 9 44	*f*	S Herculis	16 47.4	+15 7	8 *d*
R Aurigæ	5 9.2	+53 28	*f*	R Ophiuchi	17 2.0	−15 58	9.3*d*
U Orionis	5 49.9	+20 10	10.9*d*	T Herculis	18 5.3	+31 0	9.2*d*
R Lyncis	6 53.0	+55 28	8	R Scuti	18 42.2	− 5 49	6
R Gemin.	7 1.3	+22 52	9	R Aquilae	19 1.6	+ 8 5	12 *d*
S Canis Min.	7 27.3	+ 8 32	8 *i*	R Sagittarii	19 10.8	−19 29	7.5
R Cancri	8 11.0	+12 2	8	S "	19 13.6	−19 12	*f*
V "	8 16.0	+17 36	11 *i*	R Cygni	19 34.1	+49 58	10 *i*
S Hydrae	8 48.4	+ 3 27	9	RT "	19 40.8	+48 32	6.5 *i*
T "	8 50.8	− 8 46	9 *i*	X "	19 46.7	+32 40	6 *i*
R Leo. Min.	9 39.6	+34 58	8	S Cygni	20 3.4	+57 42	*f*
R Leonis	9 42.2	+11 54	9 *d*	RS "	20 9.8	+38 28	7 *i*
R Urs. Maj.	10 37.6	+69 18	12.5*d*	R Delphini	20 10.1	+ 8 47	12.4*d*
R Comae Ber.	11 59.1	+19 20	*u*	U Cygni	20 16.5	+47 35	9 *d*
T Virginis	12 9.5	− 5 29	*u*	V "	20 38.1	+47 47	13 *d*
R Corvi	12 14.4	−18 42	*u*	T Aquarii	20 44.7	− 5 31	11.5*d*
Y Virginis	12 28.7	− 3 52	*u*	R Vulpec.	20 59.9	+23 26	11.5*d*
T Urs. Maj.	12 31.8	+60 2	13 *d*	T Cephei	21 8.2	+68 5	9.5*d*
R Virginis	12 33.4	+ 7 32	*u*	S "	21 36.5	+78 10	10
S Urs. Maj.	12 39.6	+61 38	9.5 *i*	S Lacertae	22 24.6	+39 48	12.5
U Virginis	12 46.0	+ 6 6	*u*	R "	22 38.8	+41 51	13 *d*
V "	13 22.6	− 2 39	*u*	S Aquarii	22 51.8	−20 53	13 *d*
R Hydrae	13 24.2	−22 46	*s*	R Pegasi	23 1.6	+10 0	9.4*d*
S Virginis	13 27.8	− 6 41	*s*	S "	23 15.5	+ 8 22	9.2 *i*
R Can. Ven.	13 44.6	+40 2	11.5*d*	R Aquarii	23 38.6	−15 50	9.7 *i*
S Bootis	14 19.5	+54 16	11 *d*	R Cassiop.	23 53.3	+50 50	*f*

NOTE:—*f* denotes that the variable is probably fainter than the magnitude

13; *i*, that the light is increasing; *d*, that the light is decreasing; *s*, that it is near the Sun; *u*, that its magnitude is unknown.

From observations made at the Halsted, McCormick and Harvard Observatories.

Variable Stars of Short Period not of the Algol Type.

	Minimum.		Maximum.			Minimum.		Maximum.	
	d	h	d	h		d	h	d	h
X Sagittarii	Jan. 1	4	Jan. 4	1	S Triangulum	Jan. 15	8	Jan. 17	10
T Vulpeculae	1	7	2	16	RV Scorpii	15	12	16	22
SU Cygni	1	9	2	17	TX Cygni	15	17	20	20
V Velorum	1	17	2	16	W Virginis	16	5	24	10
V Centauri	2	1	3	12	δ Cephei	16	6	16	15
W Geminorum	2	10	5	1	β Lyræ	16	6	19	8
S Triangulum	2	17	4	19	SU Cygni	16	18	18	2
S Normae	2	19	7	5	T Velorum	17	3	18	12
R Crucis	3	4	4	13	S Crucis	17	22	19	10
T Velorum	3	5	4	14	W Geminorum	17	22	20	13
β Lyræ	3	8	6	10	T Crucis	18	3	20	4
RV Scorpii	3	9	4	19	T Monocerotis	18	23	26	21
ξ Geminorum	3	17	8	17	T Vulpeculae	19	1	20	10
S Crucis	3	20	5	8	V Velorum	19	5	20	4
T Crucis	4	16	6	17	V Carinæ	19	18	21	22
S Muscae	4	21	18	8	W Sagittarii	20	7	23	7
W Sagittarii	5	2	8	2	SU Cygni	20	14	21	22
SU Cygni	5	5	6	13	R Crucis	20	15	22	0
δ Cephei	5	12	6	21	RV Scorpii	21	14	23	0
T Vulpeculae	5	17	7	2	δ Cephei	21	15	23	0
V Velorum	6	2	7	1	S Triangulum	21	16	23	18
V Carinae	6	9	8	13	T Velorum	21	18	23	3
X Cygni	7	5	13	10	X Sagittarii	22	6	25	3
T Velorum	7	20	9	5	S Normae	22	7	26	17
X Sagittarii	8	5	11	2	S Crucis	22	14	24	2
S Crucis	8	13	10	1	β Lyrae	22	17	26	0
Y Ophiuchi	8	15	14	20	T Vulpeculae	23	11	24	20
R Crucis	9	0	10	9	V Velorum	23	13	24	12
SU Cygni	9	1	10	9	X Cygni	23	14	29	19
S Triangulum	9	1	11	3	V Centauri	24	1	25	13
RV Scorpii	9	11	10	21	ξ Geminorum	24	1	29	1
β Lyræ	9	19	13	2	S Muscae	24	3	27	14
T Vulpeculae	10	4	11	13	SU Cygni	24	11	25	19
W Geminorum	10	4	12	19	T Crucis	24	20	26	21
V Velorum	10	11	11	10	W Geminorum	25	16	28	7
δ Cephei	10	19	12	4	Y Ophiuchi	25	18	31	23
T Crucis	11	9	13	10	T Velorum	26	10	27	19
T Velorum	12	12	13	21	R Crucis	26	11	27	20
S Normae	12	13	16	23	V Carinæ	26	11	28	15
W Sagittarii	12	17	15	17	δ Cephei	27	0	28	9
SU Cygni	12	22	14	7	S Crucis	27	7	28	19
V Centauri	13	1	14	12	RV Scorpii	27	15	29	1
V Carinæ	13	2	15	6	V Velorum	27	22	28	21
S Crucis	13	5	14	17	T Vulpeculae	27	22	29	7
ξ Geminorum	13	21	18	21	S Triangulum	28	0	30	2
S Muscae	14	13	18	0	SU Cygni	28	7	29	16
T Vulpeculae	14	14	15	23	β Lyræ	29	4	32	6
V Velorum	14	20	15	19	X Sagittarii	29	6	32	3
R Crucis	14	20	16	5	TX Cygni	30	11	35	14
X Sagittarii	15	5	18	2	T Velorum	31	1	32	10

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Greenwich Mean Time.]

U Cephei.

	d	h
Jan.	1	6
	3	17
	6	5
	8	17
	11	5
	13	17
	16	5
	18	16
	21	4
	23	16
	26	4
	28	16
	31	3

Z Persei.

	d	h
Jan.	1	5
	4	6
	7	8
	10	9
	13	10
	16	12
	19	13
	22	14
	25	16
	28	17
	31	19

Algol.

	d	h
Jan.	1	23
	4	20
	7	17
	10	14
	13	11
	16	7
	19	4
	22	1
	24	22
	27	19
	30	15

λ Tauri.

	d	h
Jan.	4	5
	8	4
	12	3
	16	1
	20	0
	23	23
	27	22
	31	21

R Canis Maj.

	d	h
Jan.	1	7
	2	11
	3	14
	4	17
	5	20
	7	0

R Canis Maj.

	d	h
Jan.	8	3
	9	6
	10	9
	11	13
	12	16
	13	19
	14	23
	16	2
	17	5
	18	8
	19	12
	20	15
	21	18
	22	21
	24	1
	25	4
	26	7
	27	10
	28	14
	29	17
	30	20
	31	23

V Puppis.

	d	h
Jan.	2	1
	3	12
	4	23
	6	10
	7	20
	9	7
	10	18
	12	5
	13	16
	15	3
	16	14
	18	1
	19	12
	20	23
	22	10
	23	20
	25	7
	26	18
	28	5
	29	16
	31	3

S Cancri.

	d	h
Jan.	3	9
	12	20
	22	8
	31	20

S Antliæ.

Period $7^h\ 46^m\ 48^s$

S Antliæ.

	d	h
Jan.	1	6
	2	5
	3	4
	4	4
	5	3
	6	2
	7	2
	8	1
	9	0
	10	0
	10	23
	11	22
	12	22
	13	21
	14	20
	15	20
	16	19
	17	19
	18	18
	19	17
	20	17
	21	16
	22	15
	23	15
	24	14
	25	13
	26	13
	27	12
	28	11
	29	11
	30	10
	31	9

S Velorum.

	d	h
Jan.	2	17
	8	15
	14	14
	20	12
	26	10

W. Urs. Maj.

Period $4^h\ 00^m\ 12.8^s$

Jan. 1-31 12^h

RR Velorum.

	d	h
Jan.	2	3
	3	23
	5	20
	7	16
	9	13
	11	9
	13	6
	15	2
	16	23
	18	19

RR Velorum.

	d	h
Jan.	20	16
	22	12
	24	9
	26	6
	28	2
	29	22
	31	19

Z Draconis.

	d	h
Jan.	1	23
	3	8
	4	17
	6	1
	7	10
	8	18
	10	3
	11	12
	12	21
	14	6
	15	14
	16	22
	18	7
	19	16
	21	0
	22	9
	23	18
	25	2
	26	11
	27	19
	29	4
	30	13
	31	21

δ Libræ.

	d	h
Jan.	1	19
	4	3
	6	11
	8	18
	11	2
	13	10
	15	18
	18	2
	20	10
	22	18
	25	1
	27	9
	29	17

U Coronæ.

	d	h
Jan.	2	1
	5	12
	8	23
	12	10
	15	21
	19	8
	22	19

U Coronæ.

	d	h
Jan.	26	5
	29	16

R Aræ.

	d	h
Jan.	2	11
	6	21
	11	7
	15	17
	20	4
	24	14
	29	0

U Ophiuchi

	d	h
Jan.	1	13
	2	9
	3	5
	4	1
	4	21
	5	18
	6	14
	7	10
	8	6
	9	2
	9	22
	10	18
	11	14
	12	11
	13	7
	14	3
	14	22
	15	19
	16	15
	17	11
	18	7
	19	4
	20	0
	20	20
	21	16
	22	12
	23	8
	24	4
	25	0
	25	21
	26	17
	27	13
	28	9
	29	5
	30	1
	30	21
	31	18

Z Herculis.

	d	h
Jan.	1	3
	3	10
	5	13
	7	10

Minima of Variable Stars of the Algol Type.—Continued.

Z Herculis.			SW Cygni.			W Delphini.			VV Cygni.			Y Cygni		
	d	h		d	h		d	h		d	h		d	h
Jan.	9	13	Jan.	26	7	Jan.	20	13	Jan.	24	7	Jan.	11	10
	11	10		30	21		25	9		25	19		12	20
	13	12					30	4		27	6		14	9
	15	9	UW Cygni.							28	17		15	20
	17	12	Jan.	4	2	VV Cygni.				30	5		17	9
	19	9		7	13	Jan.	2	3		31	16		18	20
	21	12		11	00		3	15	VW Cygni.				20	9
	23	9		14	11		5	2	Jan.	8	13		21	19
	25	12		17	22		6	14		17	0		23	9
	27	9		21	8		9	13		25	10		24	19
	29	12		24	19		11	0					26	9
	31	0		28	6		12	11	Y Cygni.				27	19
SW Cygni.				31	17		13	23					29	9
Jan.	3	10	W Delphini.				15	10	Jan.	2	10		30	19
	8	0	Jan.	1	8		16	22		3	20	UZ Cygni.		
	12	14		6	3		18	9		5	10	Jan.	24	16
	17	3		10	23		19	21		6	20			
	21	17		15	18		21	8		8	10			
							22	20		9	20			

New Variable 156. 1904 Ceti.—In *A. N.* 3975 the following estimates of the brightness of this variable are given by Dr. W. Luther, of Düsseldorf, and Mr. P. Moschik, of Königstuhl-Heidelberg, the latter using a Zöllner photometer:

		Mag.				Mag.	
Sept.	20	9.0	Luther	Oct.	14	10.1	Moschik
	30	9.7	"		16	10.6	"
Oct.	3	9.8	"		19	10.9	"
"	13	9.9	Moschik				

New Variable Stars 157, 158, 160 and 161. 1904.—These are announced by Professor Ceraski in *A. N.* 3971, as discovered by Mme. Ceraski upon the photographs taken at Moscow. Their positions are

No.	R. A. 1855.0			Decl. 1855.0		R. A. 1900.0			Decl. 1900.0	
	h	m	s	°	′	h	m	s	°	′
157. 1904	5	53	05.9	+ 46	15.8	5	56	27.0	+ 46	16.1
158. 1904	17	48	20	+ 7	52	17	50	30	+ 7	51
160. 1994	23	39	15	+ 53	42	23	41	26	+ 53	57
161. 1904	19	51	27.4	+ 26	10.2	19	53	19.5	+ 26	17.3

The first is BD. + 46° 1089, magnitude 9.5. In April 1904, according to five observations by Mr. Blajko, it was about 9.2^m; in August and September it increased slowly from 10.5^m Aug. 15, to 9.5 Sept. 7. The period is unknown.

The second is a faint star, which on several photographs taken in 1899-1904 ranges from 10 to 12½ magnitude.

The third has about the same range of variation.

The fourth is BD. + 26° 3741, magnitude 9.1. The photographs indicate a quite rapid variation between 9.5 and 10.5 in a period of a few days. The star is reddish in color and Mr. Blajko noted it on Oct. 2 and 3 as of 8.2 and 8.8 magnitude.

New Variable Star 159. 1904 Pegasi.—This is the one noted last month as discovered by Williams, and as possibly a new star. Investigation appears to indicate that it is a long period variable. Its position has been

determined at Rome and at Arcetri, the mean of four results being R. A. 1904.0 $22^h\ 21^m\ 12^s.61$, Decl. 1904.0 $+ 29°\ 59'\ 05.1''$.

New Variable 162. 1904 Herculis.—In Circular No. 87 of Harvard College Observatory, Professor Pickering calls attention to this variable discovered by himself with the meridian photometer. He says: "The meridian photometer, like other meridian instruments, is not adapted to the discovery of variable stars. It may therefore be of interest to note the discovery of such an object by the writer, with the 12-inch meridian photometer. On August 23, 1904, while measuring the star BD. + 24° 3419, mag. 9.4, it was noticed that a brighter star, having the photometric magnitude 9.5 and not in the Bonn Durchmusterung, preceded it. An examination the next day, of the photographs of this region, at once showed that the star was a variable of long period having a range extending at least from the magnitude 9.5 to <13. The approximate position for 1855 is R. A. $18^h\ 20^m\ 26^s.0$ Decl. + 24° 56′.4."

Two New Variables 163. 1904 Lacertae and 164. 1903 Cygni.—These are announced in *A. N.* 3975 by Professor Ceraski. Their positions are

	R. A. 1855			Decl. 1855		R. A. 1900			Decl. 1900	
	h	m	s	°	′	h	m	s	°	′
163. 1904	22	01	15	+ 37	02	22	03	11	+ 37	15
164. 1904	21	42	57	+ 43	33	21	44	43	+ 43	46

The first seems to vary between 9.5 and 12.5 magnitude. On a photograph December 5, 1898 it was estimated at 10^m, October 2, 1899, 10^m, August 23, 1900, 9.5^m, October 3, 1900, 11^m. On photographs August 19, 1901, October 1, 1902, and September 7, 1904 the brightness was below 12.5.

The second also varies from 9^m at maximum to 12.5, if not lower, at minimum, but no definite minimum has been observed.

Maxima of Y Lyræ.

Period $12^h\ 03.9^m$. The minimum occurs $1^h\ 40^m$ before the maximum.

	d	h		d	h		d	h		d	h
Jan.	1-7	14	Jan.	8-14	15	Jan.	15-22	16	Jan.	23-30	17

Maxima of RZ Lyræ.

Period $12^h\ 16^m\ 15^s.0$.

	d	h		d	h		d	h		d	h
Jan.	1	10	Jan.	9	14	Jan.	17	18	Jan.	25	22
	2	10		10	14		18	19		26	23
	3	11		11	15		19	19		27	23
	4	11		12	15		20	20		29	0
	5	12		13	16		21	20		30	0
	6	12		14	17		22	21		31	1
	7	13		15	17		23	21			
	8	13		16	18		24	22			

Maxima of UY Cygni.

Period $13^h\ 27^m\ 27^s.59$. The minimum occurs $1^h\ 55^m$ before the maximum

	d	h		d	h		d	h		d	h
Jan.	2	0	Jan.	9	21	Jan.	17	17	Jan.	25	14
	3	3		11	0		18	20		26	17
	4	6		12	3		19	23		27	19
	5	9		13	6		21	2		28	22
	6	12		14	8		22	5		30	1
	7	15		15	11		23	8		31	4
	8	18		16	14		24	11			

GENERAL NOTES.

Gold Medal Awarded for Dr. Brooks' Cometary Discoveries.—Hobart College, Geneva, N. Y., and Dr. Brooks, Professor of Astronomy in the College, have both been honored at the St. Louis World's Fair.

The International Jury have awarded a bronze medal to Hobart College for its general exhibit in higher education, representing all departments; and to the astronomical exhibit, prepared by Professor Brooks, a special gold medal. The distinguishing feature of the Astronomical exhibit, and which was warmly commended by the Jury, was a photographic collection of all the comets, now twenty-four in number, discovered by Professor Brooks. Eleven of these discoveries, as many of our readers know, were made at the Red House Observatory, and thirteen at the Smith Observatory, Geneva, N. Y.

The Latitude and Longitude of the University of Wooster Observatory, Wooster, Ohio, have been made recently by W. H. Wilson and are as given below:

Lat. = 40° 48′ 38″ North
Long. = 5^h 27^m $44^s.3$ West of Greenwich.

Signaling to Mars.—Mr. Larkin of Mt. Lowe has written a letter to the *Examiner* of Los Angeles, California about signaling to the planet Mars. He makes a good point, and one that is often overlooked, when he says that the dark or night side of two planets are never turned toward each other. When the Sun is between them it is day on the side of Mars which is towards us, and also day on the side of the Earth which is towards Mars. When they are on the same side of the Sun it is day on Mars, when night on the Earth and for this reason they could not see our signals.

This should make it apparent that the task of signaling to Mars is a more difficult one than the most hopeful theorist has probably considered. All this is under the supposition that the Martians (if there are such) are beings like ourselves. If they are not like us, we cannot guess what they are like.

Rock Pressure at Great Depths.—From recent correspondence found in *Nature*, Oct. 20, 1904, p. 602, it appears that some promineut physicists have been considering the rock pressure that would effect the drilling if it were extended in the Earth to the depth of twelve miles.

In this matter Mr. Charles A. Parsons made reference to "the Philosophical Transactions of the Royal Society of England, part i, 1882, in which the great shearing stresses that are thrown on the Earth's structure by the weight of mountain ranges on elevated continents and the great depths of the sea are exhaustively treated." Mr. Parsons goes on to say:—"I would only point out that such stresses have been endured for long epochs, and that in view of the established fact that rocks are viscous, it is clear that much greater stresses could be sustained for the comparatively short time necessary to complete a deep shaft boring.

It would however be interesting to subject a cylinder of granite or quartz rock, carefully fitted into a steel mould and having a small hole bored through its center, to a shrinkage, say 100 tons per square inch, and see what shrinkage in the hole would result, or a hole might be bored into the specimen through an aperture in the mould while subjected to the pressure. This pressure would correspond to a depth of about thirty-eight miles.

This study is one of great interest both to the physicist and to the astronomer.

Orionids at University of Va.—The Orionid meteors were observed here on Oct. 14, 16 and 18 by myself, and on Oct. 18 also by Mr. J. Brookes Smith and Mr. J. P. Smith of this University, observing from Dudley's Mt., distant eight miles to the S. W. The following tables give the result of the counts:

Date					Orionids.	Others.	Total.	Remarks.
1904 Oct. 14	13	22	— 14	0	3	3	6	Fairly clear.
	14	0	— 15	2	2	15	17	" "
Oct. 16	12	23	— 13	23	5	8	13	Very clear
	13	23	— 14	23	6	11	17	" "
	14	49	— 15	29	6	1	7	" "
Oct. 18	11	16	— 12	0	2	1	3	Moon and smoke.
	12	0	— 13	0	2	3	10	" "
	13	0	— 14	0	7	7	14	" "
	14	0	— 15	0	12	5	17	Clear
	15	0	— 16	0	11	5	16	"
	16	0	— 17	16	12	3	15	"
Total					68	67	135	

OBSERVER C. P. O.

1904 Oct. 18	11	56	— 13	0	3	2	5	Moon.
	13	0	— 14	0	2	4	6	"
	14	0	— 15	0	11	6	17	Clear.
	15	0	— 16	0	9	2	11	"
	16	0	— 16	22	7	2	9	"
	Times not recorded				0	12	12	
Total					32	28	60	

OBSERVER J. B. S.

1904 Oct. 18	11	52	— 13	0	4	1	5	Moon.
	13	0	— 14	0	3	2	5	"
	14	0	— 15	0	15	2	17	Clear.
	15	0	— 16	0	6	3	9	"
	16	0	— 16	32	9	3	12	"
	Times not recorded				0	7	7	
Total					37	18	55	

OBSERVER J. P. S.

The shower was remarkable for the small number of bright meteors seen, and on the night of the maximum it was less conspicuous than for several years past, except in 1902 when moonlight interfered. The radiants of the Orionids on the several nights were as follows:

Date.	α	δ	No. Meteors.	Observer.
1904 Oct. 14	97°	+ 17°	4	C. P. O.
16	93	+ 18	6	C. P. O.
18	92	+ 16	22	C. P. O.
18	91	+ 16	11	J. B. S.
18	92	+ 16	5	J. P. S.
18	94	+ 19	4	J. P. S.
18	90	+ 13	4	J. P. S.

Only meteors in favorable positions and well observed were used to determine the above radiants. On Mr. J. P. Smith's map for October 18, there are three distinct radiants, all sharply defined. On the maps of the other observers for the same date, the single positions given satisfy most of the meteors well, but there are a few besides which would radiate from both the lesser radiants.

The shift in the radiants as shown by my observations is small but decided.

However, unfortunately, those for October 14 and 16, depend on too few meteors to be conclusive, though the probability is strong that a real change has been detected.

The most brilliant meteor was No. 4341, seen at $13^h\ 14^m\ 20^s$ on October 18. It was fully equal to Venus, of an orange color, but did not belong to the Orionid group.

On Oct. 18 about thirty-five meteors were observed at both stations, and the real paths of a large proportion of them will be computed and the results communicated later.

CHARLES P. OLIVIER.

STUDENTS OBSERVATORY,
University of Va.

British Astronomical Association Report of a Year's Work.—From the last report covering a period from Oct. 1, 1903 to Sept. 30, 1904, some interesting facts are learned. At the beginning of the year, the association had a membership of 1,056, at the end of the year it is 1,007, a loss of forty-nine members, chiefly from deaths and resignations. This large and active association has kept up its good record in the matter of publications. In addition to the usual issue of the "*Journal*" of the association, it has published several creditable memoirs. Volume XI. is now complete which is occupied with the work of the Variable Star section. Part 2, of Volume XII of the Solar section, part 3, of the Jupiter section and part 1. Volume XIII of the Meteor section. The observations of the Solar section were concerned with the great sun-spot of Oct. 1903, and the simultaneous displays of aurora borealis, but the chief work of observation and of record has been on the life histories of sunspots, including visual, magnetic and spectroscopic features.

From these observations it appears that a general disturbed state of the Sun corresponds to a period of magnetic storms, but no relation between isolated spots or prominences and magnetic disturbances yet appears. The connection is not one of cause and effect.

In the spectra of sun-spots, the prevalence of titanium flutings in the spectra of Secchi's third type would seem to suggest that such stars are suns having very spotted photospheres. Vanadium and titanium are found to be the elements whose lines are most affected in sun-spots.

The work of the Lunar section has been mainly to observe for changes in the surface markings of the Moon. Professor W. H. Pickering's work during the last year has awakened interest in this section. Its observations are especially directed to suspected changes in the floor of Plato.

The observations of the Mercury and Venus section were limited. In the work on Mercury only two nights of fine definition were found. Then no markings were seen on the planet's surface; at another time a shaded, but a fairly well defined area was seen. The seeing at the time these observations was esteemed good. The terminator on Mercury was much less shaded than on Venus under almost similar conditions.

In the report of the Saturn section attention is called to the observation of the new white spot seen first almost simultaneously by Mr. Barnard of Yerkes Observatory and Mr. Denning of England. It was situated in about 30° north latitude of the planet. The spot was unusually far from the equator, for such a marking and it was at once observed very carefully and very generally to determine the period of rotation of the planet. The result obtained from several sources, in which there was close agreement, was ten hours and thirty-eight min.

utes for the rotation-time of the planet in that latitude. The period heretofore accepted is ten hours and fourteen minutes, which was obtained by observing spots near the planet's equator. The inference is that portions of the planet's surface in different latitudes have different times of rotation. Mr. Denning observed Saturn for sixty-five nights.

The comet section has had little to report because, within its year, only one comet was discovered and that was found April 16, by Professor Brooks of Geneva, in this country. That comet is remarkable for having been third in the list of known comets, as to perihelion distance from the Sun. At the time of discovery it had passed its perihelion place, and on this account the time for observation of it was limited.

Tempel second periodic comet is now due, but it has not yet been seen.

Encke's comet has been twice photographed at Heidelberg by A. Kopff on Sept. 11 and 17 with an exposure of three and a half hours. The elements and ephemeris are known.

The meteoric section reports that the Perseids of August were weaker than usual, although some of the nights for observation were very favorable. Brilliant meteors appeared in January, February, March, April and July and six in August.

The variable star section has had a good working list of forty-seven stars in all. It has also taken up recently observations of certain selected areas of the Milky Way, with a view to the detection of new variables.

The association has a photographic section, a library and a lantern slide department. Its work as a whole commands the attention of astronomers everywhere.

The Life History of Radium.—This most interesting substance has attracted the attention of scholars all over the world. Its many curious properties are likely to touch and influence every science, and astronomers are on the lookout for a large share. In the *Journal of the Franklin Institute* for November, the following brief account of the life history of radium is given:—

"The view that uranium is the parent substance of radium was advanced by Rutherford and Soddy on the ground that it is one of the few elements having a higher atomic weight, that it is the main constituent of radium ores, and that the proportion of radium in good pitchblende corresponds roughly with the ratio of activity of radium and uranium. An examination of a number of specimens of uranium salts purchased from seventeen to twenty-five years ago showed that these all contained a larger proportion of radium than the more modern specimens. This result is in accordance with the theory enunciated by Rutherford and Soddy, but may easily be due to modified methods of preparation. F. Soddy (*Nature*, 70, p. 30, May 12, 1904), tsates that a kilogram of uranium nitrate was purified until the proportion of radium present was less than 10^{-13} gram as tested by the maximum amount of accumulated emanation. At the end of twelve months the amount of accumulated radium was certainly less than 10^{-11} gram instead of the 5×10^{-7} gram calculated from the ratio of the radio-activities of radium and uranium. The quantity of radium produced was therefore less than one ten-thousandth part of the theoretical quantity, and this result practically settles, in a negative sense, the question of the production of radium directly from uranium. It is, of course, possible that intermediate substances might exist, and that radium would only be produced at a later stage, but there is no experimental evidence in support of this view."

Jupiter's Third Satellite Seen With the Naked Eye.—In the *Astronomische Nachrichten* No. 3975, Dr. J. Möller, of Elsfleth, recounts an

extraordinary observation of Jupiter's third satellite. He says: "On November 1, 1903 while on board a sailing vessel, in a calm ocean, in north latitude 15° and west longitude 137°, at seven o'clock in the evening (about 16ʰ Greenich mean time) I saw a faint little star close to Jupiter. Before I could call the attention of other of the ship's passengers to the phenomenon, the fourth officer taking an azimuth of Jupiter with the azimuth-compass called to me that he saw a little star close to Jupiter. Field-glass and Nautical Almanac left no doubt that it was the third satellite. The fourth moon, situated farther off on the other side, could not be distinguished with the naked eye. We were at the limit between an unusually extensive storm area and the district, just entered on that day, of the northeast passage, where according to my experience the air is apt to be especially transparent."

Leonid Meteors.—Two observers watched the sky in the region of the constellation of Leo on the morning of November 15th, from 2:35 to 4:45 Eastern Standard time, but only twenty-eight Leonids were seen. The sky was cloudless, the Milky Way bright.

With a single exception, none of the meteors seen was brighter than the second magnitude, and several were very faint. The faint ones were noticeably orange in color, the brighter ones a bluish white. They were very swift, with short trails so that it was difficult to determine their path with accuracy.

The following is a summary of the count of meteors seen:

Time.	Leonids.	Non-Leonids.
2:35-3:00	2	0
3:00-3:30	6	5
3:30-4:00	8	10
4:00-4:30	7	5
4:30-4:45	5	2
Total	28	22

Anne Sewell Young.

Mount Holyoke College,
South Hadley, Mass.

The Eros Parallax Campaign.—Circular No. 11 of the International Astrophotographic Conference of July 1900, a quarto volume of over 400 pages, has just come to hand, having been published at Paris, Oct. 1, 1904. It contains the results of micrometric measures of the planet Eros, made at the observatories at Marseilles, Padua, of heliometer measures at Bamberg, and of photographic observations of the planet or of the comparison stars at Algiers, Catania, Northfield, San Fernando, Paris and Toulouse. There is also a very important paper by Mr. Tucker of Lick Observatory containing a normal catalogue of the stars of reference based upon the published meridian observations of thirteen observatories. In an appendix is given a list of researches and observations connected with the Eros work which have been published elsewhere than in the Circulars of the Conference.

In his introductory note Professor Loewy, the Director of the Observatory of Paris, speaks with gratification of the great quantity of material which is now at hand for the parallax problem, and of the great accuracy which has been so generally attained by the forty observatories coöperating in this work. He expresses the hope that the next Circular, No. 12, will contain all of the observations of real value now remaining unpublished.

The following table gives a résumé of the photographic observations which are

contained in Circulars 10 and 11. From this it will be seen that 811 plates have been measured yielding 522 positions of the planet Eros and also extremely accurate places of about 3000 stars scattered along a part of the apparent path of Eros. These star places, aside from contributing to the problem for which they were directly intended, have a permanent scientific value.

Three lists of stars are referred to in the table:

First, the stars of reference (*étoiles de repère*). These were selected at the beginning of the campaign, to serve as reference stars in measuring the photographic plates. They were scattered along the path of Eros so that ten or twelve might be found upon any photograph taken. Their places were measured with all possible accuracy with the meridian circles at thirteen observatories. The normal positions obtained from thirteen sets of observations form the basis of the photographic measurements. The photographs in turn give revised positions of these same stars as well as of other fainter stars.

Second, the comparison stars (étoiles de comparaison) which were used in the micrometric measures of Eros made with the great telescopes at several observatories. These stars were generally faint, running even as low as the thirteenth magnitude in some cases. Their positions have been determined so far as possible from measures of the photographs, special series of plates being taken for this purpose at some of the observatories.

Third, in order to make sure of having a sufficient number of comparison stars near the planet, several observatories adopted the plan of measuring at the same time with the images of Eros, those of all the faint stars within a space 20′ square, having the planet in its center.

RÉSUMÉ OF THE PHOTOGRAPHIC OBSERVATIONS, RELATING TO THE DETERMINATION OF THE SOLAR PARALLAX, CONTAINED IN CIRCULARS NOS. 10 AND 11.

		Stars of Reference.		Stars in 20′ Zone.	Comparison Stars.		Planet.
Observatory	No. of Plates.	No. of Stars	No. of Obs.	No of Obs.	No. of Stars.	No. of Obs.	No. of Obs.
Algiers	112	631	1648	.....	929	1944	.....
Bordeaux	52	232	711	676	306	1068	52
Catania	47	434	573	2272	397	534	.....
Northfield	68	286	557	.....	22	76	68
Paris	284	505	3682	2488	748	4688	284
San Fernando	166	662	2930	1834	778	1720	76
Toulouse	82	463	2481	612	1530	2641	42
Totals	811	671	12582	7882	962	12671	522

Special Time Signal.—In speaking of the special time signal sent out from the Naval Observatory at Washington, Sept. 8, 1904, during the session of the National Geographic Congress at that place, we said, on p. 615 of our last issue, four lines from bottom: "The long intervals in a few cases are perplexing if the wire connection was continuous. If messages were repeated, at any point in transmission to Mauritius or Rio Janeiro, for example, an interval of a few minutes might be necessary."

Superintendent C. M. Chester of the Naval Observatory, upon our request, had the kindness to send us a type-written copy of a report of the results of the special time-signal, for use in this publication. The telegraphic replies which were timed covered two points: One, a message of greeting to the Geographic Congress at Washington from many different points all over the world, the other, a message

intended to give the interval of time approximately, that would be required to send a time-signal to all countries in the world, if there were but one system of time distribution in use. The data that we had in hand from Washington did not clearly distinguish in all cases between these two messages; so we were in doubt in some cases, and made the statement above referred to, in order to cover that point.

In a letter from Superintendent Chester under date of Nov. 5, he speaks of the delay incident to manual repeating over each branch of a long cable line, and says that a time-signal from Washington around the world would require sixteen repetitions, as follows: New York, Azores, Lisbon, Gibralter, Alexandria, Suez, Aden, Bombay, Madras, Singapore, Hongkong, Manila, Guam, Midway, Honolulu and San Francisco.

It is very evident that manual repetitions of a message would consume time, and the great wonder is that all the messages reported were conveyed so far, in such way in so short a time. We did not mean to convey the idea that any of these messages were so accurately timed as to give the true time interval of the actual transmission of a message between any two points of the vast territory covered. Any one acquainted with this kind of work would know that such a thing would be impossible under the circumstances.

The report of this special time-signal that appeared in the *National Geographic Magazine* for October, p. 411, gives information on the point of time-intervals between Washington and other points which is a fairly close approximation in regard to time of transmission when repeaters and "wave and armature movements are included in the calculation. We copy some of these results:

Source.	Place.	Interval.
Cordoba Obs'y	Argentina, S. A.	2. seconds
National "	Tacubaya, Mexico	0.36 "
McGill "	Montreal, Canada	0.10 "
Meteorological Obs'y	Toronto	0.23 "
Harvard Obs'y	Cambridge, Mass.	0.10 "
Lick "	Mt. Hamilton, Cal. (fast)	0.24 "
Goodsell "	Northfield, Minn.	0.10 "
Washburn "	Madison, Wis.	0.30 "
Chamberlain Obs'y	Denver, Colo.	0.07 "
Laws Obs'y	Columbia, Mo.	0.54 "
Allegheny Obs'y	Allegheny, Pa.	0.42 "

In our previous article, we inadvertently omitted to give the time interval at the National Observatory at Tacubaya, Mexico, and we wrongly placed that important observatory at the City of Mexico. It is located at Tacubaya, Mexico.

We have recently received a letter from the Director of the National Observatory at Tacubaya showing the extreme care used in getting the time interval given above. He also calls attention to our report of the time-interval from his observatory on last New Year's eve. It was reported from the *Washington Circular* as one minute and nineteen hundredths seconds. The true interval was 0.19 seconds. It was probably a misprint in the *Washington Circular.*

Halley's Comet.—Your readers may be interested in the quotations given below:—

"The first comet that has been calculated solely from European observations was that of 1456, known as Halley's comet, from the belief long, but erroneously entertained, that the period when it was first observed by that astronomer was its first and only well attested appearance." (*Cf.* Humboldt's "Cosmos"

translated by E. C. Otté, vol. 1., pp. 84, 85, note, London, 1901; also "Kosmos," in German, Erster Band, Anmerkungen, pp. 389, 390 (12), Stuttgart, 1845.)

"Halley knew that he could not himself live to witness the fulfillment of his prediction, but he says: 'If it [the comet] should return, according to our predictions, about the year 1758, impartial posterity will not refuse to acknowledge that this was first discovered by an Englishman.' This was, indeed, a remarkable prediction of an event to occur fifty-three years after it had been uttered. The way in which it was fulfilled forms one of the most striking episodes in the history of astronomy. The comet was first seen on Christmas Day, 1758, and passed through its nearest point to the Sun on March 13, 1759. Halley had then been lying in his grave for seventeen years, yet the verification of his prophecy reflects a glory on his name, which will cause it to live for ever in the annals of astronomy." (*Cf.* 'Great Astronomers,' Sir Robert S. Ball; also *Good Words*, xxxvi., pp. 753, 4.)

"It is to be hoped, the present age will not forfeit the character of impartiality; but that all the world will now unite in doing justice to such distinguishing merit." (*Cf.* 'Two Lectures on Comets,' John Winthrop, Appendix, p. 40, Boston, 1759.)

Halley, in the use of the word "this" (the original version is not in the Chicago libraries) undoubtedly referred not to his discovery of the comet of 1682, itself, but to his discovery of its identity, for as is well known, he had calculated the orbits of twenty-four comets, among which he fonnd at least three that from the similarity of their elements, he concluded to be identical with each other and with the one of 1682 which now so justly bears his name.

EUGENE FAIRFIELD MCPIKE.

CHICAGO, Illinois,
Nov. 9th, 1904.

Revision of the Cape Photographic Durchmusterung.—A part of the annals of the Cape Photographic Durchmusterung Vol. IX. has been published, consisting of divisions I, II. and III. These parts are on An Examination of questions which have arisen from a comparison of other star catalogues with the C. P. D.; Observations of Variable Stars; and Errata of southern Star Catalogues. The first part is useful in giving information about the disagreement of catalogues in regard to star magnitudes. This fact gives difficulty to those who want to know whether or not a given star is variable. The stars of the Cordoba Zone Catalogue, some of them, suggest an interesting field for study. A number of them can no longer be found, some of them are variables, and some of them turn out to be observations of minor planets, but there is no explanation for the disappearance of so many of them. Gould was an accurate observer, and Mr. Innis who has had this work in charge has confidence in his work, and has therefore given the more careful attention to the study of comparisons in which the Cor-doba Catalogue was concerned. As a result new variables have been discoveredand long period variables suspected.

In regard to Gilliss' Southern Circumpolar Catalogue, it is said that a considerable number of these stars could not be found by the aid of a 7-inch telescope. The explanation given by the Washington authorities at the Naval Observatories was that most of the discrepancies are misprints or errors which admit of corrections now made. The lists of the missing stars and the errors are given in the volume.

The part of this volume devoted to variable stars is interesting and useful in several ways. It is a record of work at the Cape covering a period from 1896 to

1902. Lists of suspected variables are easily made, but it is quite another thing to find out the reasons for discordances. That means continued study for a longer time. The curves and the star charts accompanying each variable is just what the observer needs for his information in testing the conclusions reached by those who have made a study of the same variables.

The considerable lists of errors that have been compiled for this volume is another very serviceable part of it.

On Some Results Obtained by the D. O. Mills Expedition to the Southern Hemisphere.*—In the extended program for determining the velocity of the solar system through space by means of the radial velocities of the stars, which has been in progress at the Lick Observatory for seven years, the need had long been felt for extending the scope of the work so as to cover the entire sky. For a full and rigorous solution of the problem it seemed absolutely imperative that the neglected portion of the southern sky within 60° of the South Pole be included. The generosity of Mr. D. O. Mills made it possible to supply this deficiency. As is well known, the equipment sent to South America consisted of a powerful three-prism spectrograph attached to a 37-inch reflector of the Cassegrainian form. The Observatory is situated on the summit of Cerro San Cristobal in the city of Santiago, Chile, and definite work on the program was commenced on September 11, 1903, by Astronomer W. H. Wright and Dr. H. K. Palmer. Up to June 1, 1904, three hundred and eight successful spectrograms had been secured.

One of the most interesting "by-products" of the spectroscopic determination of the solar velocity, as carried out at Lick Observatory, has been the discovery that at least one in every seven or eight of the brighter stars are spectroscopic binaries. Similar results are being secured at the Southern Station, and in *Lick Observatory Bulletin*, No. 60, Mr. Wright announces the binary character of five stars: β Doradus, W Velorum, λ Carinal, κ Pavonis, and τ Sagittarii.

Mr. Wright has also succeeded in measuring the difference in radial velocity of the components of the visual binary α Centauri. From a combination of these data with the visual elements, as is well known, the parallax can be obtained with great accuracy and without the assumptions as to the great distance of the comparison-stars used which must be made in heliometrically determined parallaxes.

The values secured are:—

$$\pi = 0''.76 \pm 0''.03$$
$$a = 3\ .46 \times 10^8$$
$$m_1 + m_2 = 1\ .9$$

Gill and Elkins's value from heliometer observations was $0''.75 \pm 0''.01$, relative to the comparison-stars used, which were of average magnitude 7.6.

H. D. CURTIS.

An Introduction to the Study of Spectrum Analysis.—W. Marshall Watts of London is the author of this new elementary text for elementary study of Spectrum Analysis. It is published by Longmans, Green & Company 39 Paternoster Row, London, and New York. The purpose of this new book is to fill the place of a text-book preparatory to the heavier standard books that deal with spectroscopy too much in detail, for the wants of professional investigators. It aims to meet the wants of students at work in this new and delightful of study: It explains how a spectrum is produced; the flame spectra, and those obtained by electricity, absorption spectra, and by the electric arc. The production of the diffraction spectrum and the measurement of wave-lengths with the many cuts for illustration is a valuable chapter. The study of the dark

* Abstract of *Lick Observatory Bulletin*, No. 60.

lines by absorption and the Fraunhofer lines of the solar spectrum is another that fills its place well. The spectra of the stars and nebulae are briefly and plainly described and amply illustrated. We are interested to see so much said in a clear and terse way. The illustrations of the bright nebulæ which astronomers know so well do not seem to the writer over abundant for the general student of this branch; for he is not expected to be very familiar with them.

The spectroscopic study of the Sun is another theme of the greatest interest at the present time. In the twelve pages devoted to this, there are fourteen cuts, most of them good illustrations for the points to be shown by them. In another chapter new stars, double stars and comets are grouped, and their spectra shown. The illustrations are from photographs of the best, recent work.

The concave grating and the spectra secured by this most helpful means makes one of the most interesting chapters which the book contains. The writer has marked this for another and a more careful reading. The last three chapters are occupied with the relations between the different lines of a spectrum and between lines of the spectra of allied elements; band spectra, spectra of compounds; and the Michelson Echelon diffraction grating. The reading of these chapters requires thought and study.

This new book has value as a reference book for its full catalogue of spectra. Its tables are those of the Rowland standard wave-lengths.

Professor Watts has prepared a very much needed book and he has done his work admirably.

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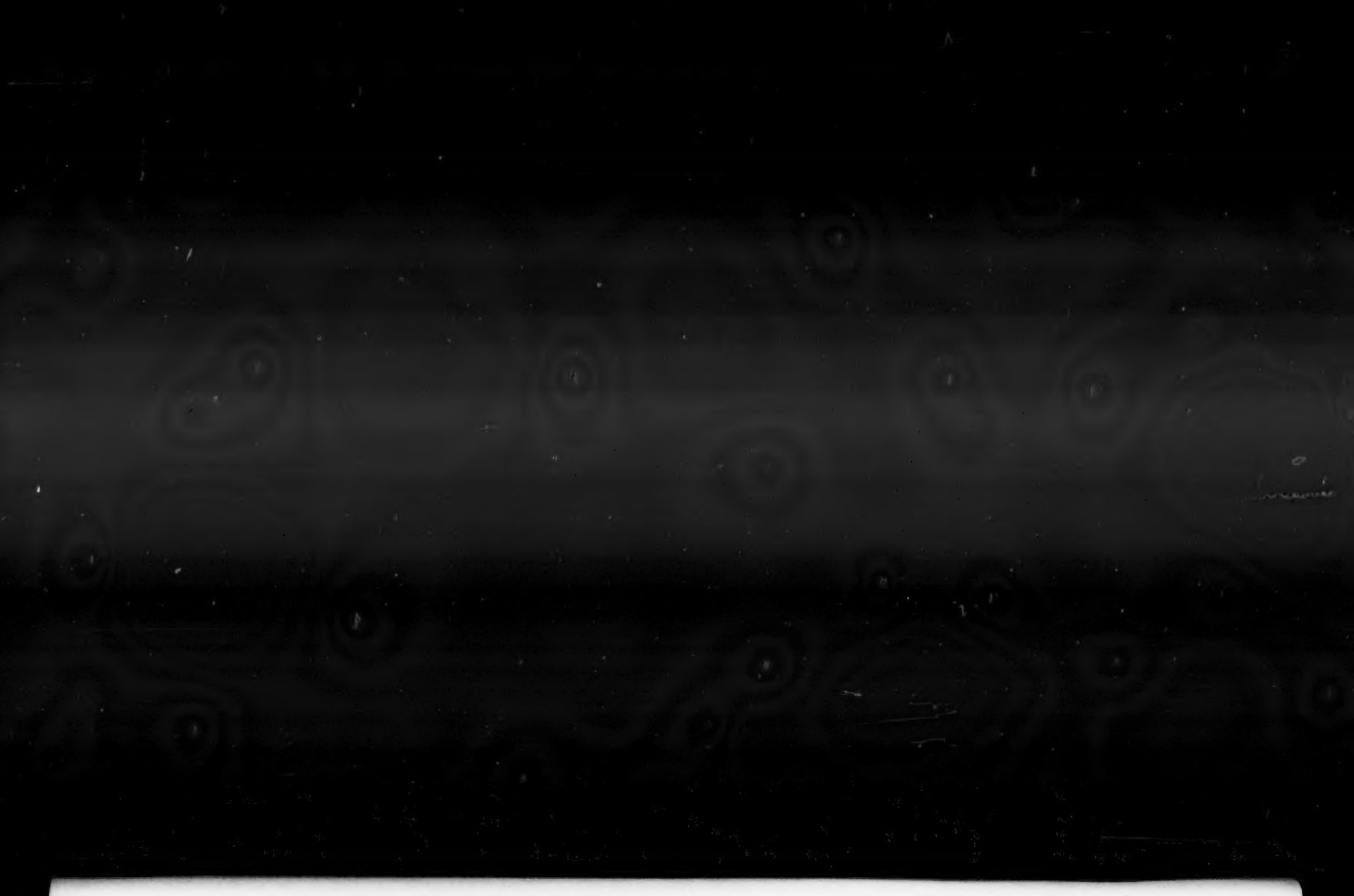
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